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VOL. XXIII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

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The Engravings consist of 1. Knight's new Method of Training Fruit Trees: 2. Mr. Cleall's Machine for Beating out the Seed of Hemp and Flax: 3. Mr. Bond's Machine for Breaking Hemp: 4. Mr. Samuel Clegg's Apparatus for making Carburetted Hidrogen Gas from Pitcoal: 5. His Lamp for burning the Gas: 6. Different Insects, called Wireworms, that destroy Wheat: 7. The Nasal Membranes of two Species of Horseshoe Bat: 8. Figures to illustrate the Vivification of Seeds, by Mrs. A. Ibbetson: 9. A very sensible Hygrometer, by Lieutenant Henry Kater: 10. An improved Hygrometer, by the fame Gentleman: 11. Mr. Davy's Apparatus for heating Potassium in Gasses, Distilling Potassium, and taking the Voltaic Spark in Sulphur and Phosphorus: 12. Various Figures by Mrs, Agnes Ibbetson, to illustrate the Growth of Leaves, and the Divisions of the Wood in the Stem of Trees: 13. Figures showing the Line of Life in Trees entering into Flower Buds, passing by Leaf Buds, and avoiding an injured Part: 14. Dissections of Seed Vessels: 15. Cryptogamian Plants, that have been mistaken for Perspiration on Leaves: 16. Delineations by the Camera Obscura and Camera Lucida.

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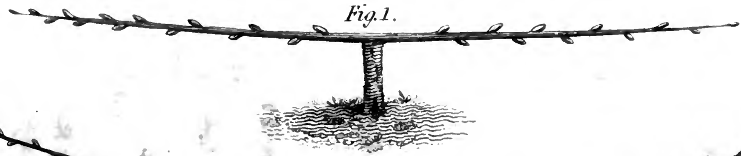

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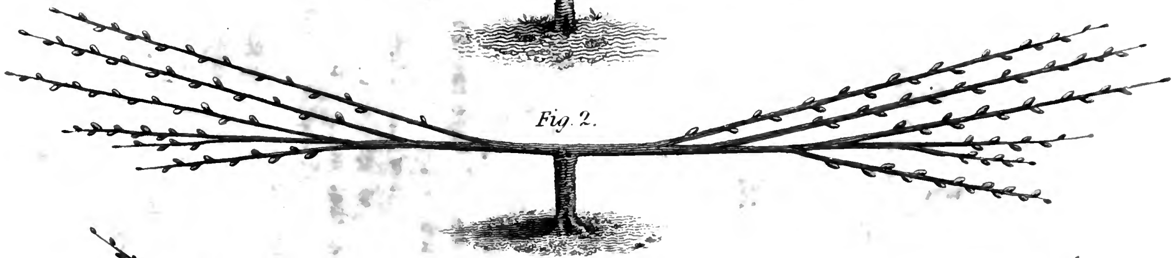

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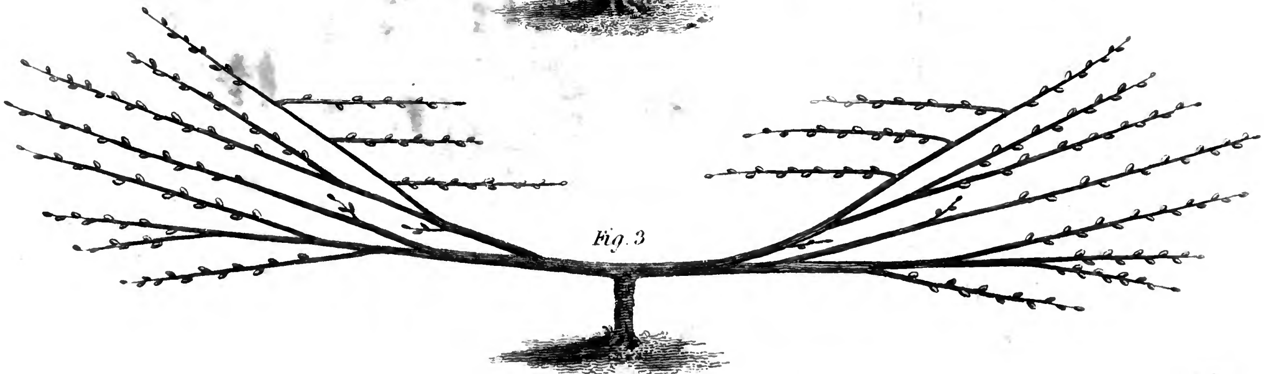

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A

JOURNAL

OF

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AND

THE ARTS.

MAY, 1809.

ARTICLE I.

On a new Method of training Fruit Trees. By THOMAS ANDREW KNIGHT, *F. R. S. &c**.

FROM the result of experiments I have made to ascertain the influence of gravitation on the descending sap of trees, and the cause of the descent of the radicle, and ascent of the expanding plumule of germinating seeds†, I have been induced to believe, that none of the forms, in which fruit trees are generally trained, are those best calculated to promote an equal distribution of the circulating fluids; by which alone permanent health and vigour, and power to afford a succession of abundant crops, can be given. I have therefore been led to try a method of training, which is, I believe, different from any that has been practised; and as the success of this method has fully an-

Usual forms of training trees defective.

A different manner tried with success.

* Trans. of the Horticultural Society, p. 79.

† Phil. Trans. for 1806 and 1807; or Journal, Vol. XIV, p. 409, and XIX, 241.

swered every expectation I had formed, I have thought a concise account of it might not be unacceptable to the Horticultural Society. I confine my account to the peach tree, though, with a little variation, the method of training and pruning, that I recommend, is applicable, even with superior advantages, to the cherry, plum, and pear tree; and I must observe, that when trees are by any means deprived of the motion, which their branches naturally receive from winds, the forms in which they are trained operate more powerfully on their permanent health and vigour, than is generally imagined.

Form of training trees when their branches are deprived of motion, important.

New method described.

My peach trees, which were plants of one year old only, were headed down, as usual, early in the spring, and two shoots only were trained from each stem in opposite directions, and in an elevation of about 5 degrees; and when the two shoots did not grow with equal luxuriance, I depressed the strongest, or gave a greater elevation to the weakest, by which means both were made to acquire and to preserve an equal degree of vigour. These shoots, receiving the whole sap of the plants, grew with much luxuriance, and in the course of the summer each attained about the length of four feet. Many lateral shoots were of course emitted from the young luxuriant branches; but these were pinched off at the first or second leaf, and were in the succeeding winter wholly destroyed; when the plants, after being pruned, appeared as represented in Pl. I, Fig. 1. This form, I shall here observe, might with much advantage be given to trees while in the nursery; and perhaps it is the only form, which can be given without subsequent injury to the tree: it is also a form that can be given with very little trouble or expense to the nurseryman.

Should be commenced in the nursery.

Second year.

In the succeeding season as many branches were suffered to spring from each plant as could be trained conveniently, without shading each other; and by selecting the *strongest* and *earliest* buds towards the points of the year old branches, and the *weakest* and *latest* near their bases, I was enabled to give to each annual shoot nearly an equal degree of vigour; and the plants appeared in the autumn of the second year nearly as represented in Fig. 2. The experienced gardener will here observe, that I exposed a greater surface of leaf

to

to the light, without placing any of the leaves so as to shade others, than can probably be done in any other mode of training; and in consequence of this arrangement, the growth of the trees was so great, that at two years old some of them were fifteen feet wide; and the young wood in every part acquired the most perfect maturity. In the winter, the shoots of the last season were alternately shortened, and left their whole length, and they were then prepared to afford a most abundant and regular blossom in the succeeding spring.

In the autumn of the third year the trees were nearly as represented in Fig. 3, the central part of each being formed of very fine bearing wood; and the size and general health of the trees afford evidence of a more regular distribution of the sap, than I have witnessed in any other mode of training. Third year.

In the preeeding method of treating peach trees very little use was made of the knife during winter: and I must remark, that the necessity of winter pruning should generally be avoided as much as possible; for by laying in a much larger quantity of wood in the summer and autumn than can be wanted in the succeeding year, the gardener gains no other advantage, than that of having a "great choice of fine bearing wood to fill his walls," and I do not see any advantage in his having much more than he wants; on the contrary, the health of the tree always suffers by too much use of the knife through successive seasons. Necessity of winter pruning should be as much as possible prevented.

To enter into the detail of pruning, in the manner in which I think it might be done with most advantage, would of necessity lead me much beyond the intended limits of my present communication; but I shall take this opportunity of offering a few observations on the proper treatment of luxuriant shoots of the peach tree, the origin and office of which, as well as the right mode of pruning them, are not at all understood either by the writers on gardening of this country, or the Continent. Remarks on pruning peach-trees.

I have shown in the Phil. Trans. for 1805*, that the alburnum, or sap wood of oak trees loses a considerable part of its weight during the period in which its leaves are formed The alburnum a reservoir of sap in winter.

* See Journal, Vol. XII, p. 253.

in

in the spring; and that any portion of the alburnum affords less extractive matter after the leaves have been formed than previously. I have also shown, that the aqueous fluid which ascends in the spring in the birch and sycamore becomes specificially heavier as it ascends towards the buds; which, I think, affords sufficient evidence, that the alburnum of trees becomes during winter a reservoir of the sap or blood of the tree, as the bulb of the hyacinth, tulip, and the tuber of the potato, certainly do of the sap or blood of these plants. Now a wall-tree, from the advantageous position of its leaves relative to the light, probably generates much more sap, comparatively with the number of its buds, than a standard tree of the same size; and when it attempts to employ its reserved sap in the spring, the gardener is compelled to destroy (and frequently does so too soon and too abruptly) a very large portion of the small succulent shoots emitted, and the apis too often prevents the growth of those which remain. The sap in consequence stagnates, and appears often to choke the passages through the small branches; which in consequence become incurably unhealthy, and stunted in their growth: and nature then finds means of employing the accumulated sap, which if retained would generate the morbid exudation, gum, in the production of luxuriant shoots. These shoots our gardeners, from Langley to Forsyth, have directed to be shortened in summer, or cut out in the succeeding spring; but I have found great advantages in leaving them wholly unshortened; when they have uniformly produced the finest possible bearing wood for the succeding year; and so far is this practice from having a tendency to render naked the lower, or internal parts of the tree, whence those branches spring, that the strongest shoots they afford invariably issue from the buds near their bases. I have also found, that the laterals that spring from these luxuriant shoots, if stopped at the first leaf, often afford very strong blossoms and fine fruit in the succeeding season. Whenever therefore space can be found to train in a luxuriant shoot, I think it should rarely or never be either cut out, or shortened: it should, however, never be trained perpendicularly, where this can be avoided.

Wall trees generate more than standards.

Luxuriant shoots should not be shortened.

II.

II.

On the Food of Plants, by the Rev. JOSEPH TOWNSHEND, *Rector of Pewsey, Wilts**.

Seeds vegetate rapidly in oxigen; and not at all in nitrogen.

WHAT is the food of plants? Before we can give a satisfactory answer to this question, we must collect facts; we must multiply experiments. For this purpose, in the years 1792 and 1793 I put various seeds to vegetate in different airs; in atmospheric air, in vital air, and in azote. The general result was that neither wheat, oats, nor barley, vegetated in azote; but in vital air vegetation was uniformly rapid.

Cabbage plants,

July 12, 1796, I placed eleven cabbage-plants in pots, all healthy plants, and weighing each ¼ ounce apothecaries' weight. The pots stood in pans with water, and remained in them till June 12, 1797, when the plants were taken out of the pots and weighed again.

Of these pots four had quartz sand, washed clean, and rendered perfectly free from mixture of either argil or calcareous earth.

in pure quartz sand,

No. 1 had nothing but this sand; the plant lived, but did not increase in bulk; when examined, the radical fibres were found numerous and extended, but very small; and when the plant was weighed in January 1797, it had not increased in weight.

sand and woollen rags,

No. 2 had the same kind of sand and woollen-rags: the roots shot vigorously, the plant cabbaged, and in January 1797 weighed two ounces.

sand and charcoal,

No. 3 had the same kind of sand, with about ¼ part charcoal in powder; the roots were less vigorous than the former, and in January 1797 the plant weighed ¾ ounce.

sand & lime,

No. 4 had this sand with about $\frac{1}{20}$ lime. The plant did not increase, yet lived, and in January 1797 weighed only 3 dwts. having lost ⅖ of its original weight.

brick clay,

No. 5 had brickmaker's clay alone; the plant lived, looked fresh, but in January 1797 weighed only ½ ounce.

clay and sand,

No. 6 had brickmaker's clay, with an equal proportion of

* Bath Society's Papers, vol. X, p. 1.

the

the quartz sand. This plant, like the fomer, lived, looked fresh, and in January 1797 weighed $\frac{1}{2}$ ounce.

clay and charcoal, No. 7 had brickmaker's clay, with about $\frac{1}{4}$ part charcoal in powder. In January 1797 the plant weighed $\frac{1}{2}$ ounce.

clay and rags, No. 8 had brickmaker's clay and woollen rags. This plant cabbaged well, and in January 1797 weighed 4 ounces.

clay and lime, No. 9 had brickmaker's clay, with about $\frac{1}{10}$ lime. The plant lived till December, but never grew.

sand and horse-dung, No. 10 had clean dung from the bowels of a horse, with quartz sand well washed. This plant dropp'd some of its largest leaves during the frost; and yet in January 1797 it weighed $4\frac{1}{2}$ ounces.

peat-earth, No. 11 had *peat* earth alone; the plant continued healthy to appearance, and in January 1797 weighed $\frac{1}{4}$ ounce, but the root was rotted off.

& rich mould. No. 12 was planted at the same time in the garden, near the pots, in rich mould: this did not drop any leaves, and in January 1797 weighed 4 ounces.

Such was the result of these experiments on cabbage plants.

Wheat sown, In January 1797, having removed the cabbage plants, I sowed wheat in the same pots; and 25th September of the same year I made the subsequent report.

in sand, No. 1, with quartz sand alone, had two stems, 23 inches long, and the ears $1\frac{3}{4}$ inch.

sand and rags, No. 2, the sand and rags, had four stems, 28 inches long, and the ears $2\frac{1}{2}$ inches.

sand and charcoal, No. 3, the sand and charcoal, had one stem, 18 inches long, and the ear $1\frac{3}{4}$ inch.

sand and lime, No. 4, the sand and lime, had two stems, 21 inches long, and the ear 2 inches.

clay, No. 5, the clay alone, had three stems, 27 inches long, and the ears $1\frac{3}{4}$ inch.

clay and sand, No. 6, the clay and sand, had four stems, 25 inches long, and the ears $2\frac{1}{2}$ inches.

clay and charcoal, No. 7, the clay and charcoal, had four stems, 24 inches, and the ears 2 inches.

clay and rags, No. 8, the clay and rags, had twelve stems, 33 inches long, and the ears $2\frac{1}{4}$ inches.

No.

No. 9, the clay and lime, had one stem very slender, 15 inches, and the ear $1\frac{3}{4}$ inch. clay and lime,

No. 10, the dung and sand, had sixteen stems, 37 inches long, and the ears $2\frac{3}{4}$ inches, very strong. dung and sand,

No. 11, the peat earth, had six stems, 35 inches long, and the ears $2\frac{1}{4}$ inches. peat earth,

Thus, it appears, that in both sets of experiments the results were similar.

Is water the food of plants?

From these facts, compared with other facts with which we are conversant; such as the flowering of bulbous roots in water, and more especially the vast increase of the withy-tree, recorded by Mr. Boyle, our attention is naturally turned in the first place to water, as the supposed nutriment of plants.

Water used with the sand.

In the experiments before us, both the cabbage and the wheat of No. 1 were well supplied with water; but in the space of six months the former had not increased in either weight or bulk; and the latter in eight months produced only two miserable stems.

A sandy soil productive.

In Catalonia, more especially in the vicinity of Barcelona, the soil is principally quartz, from decomposed granite; yet being well watered, and plentifully supplied with light and heat, the crops of every kind are most abundant.

Quality of the air as to heat and moisture important.

Mr. de Saussure remarks, that "we deceive ourselves exceedingly when we imagine, that the fertility of any district depends wholly on the nature of its soil, because abundance and scarcity in crops arise principally from the degree of heat and humidity in the air, with the quantity and quality of the exhalations with which it is charged." He adds, "I have seen, in Sicily and Calabria, rocks and gravel arid and uncultivated, such as in Switzerland would have been altogether barren, which there produced more vigorous plants than are to be seen on the richest and best cultivated lands amongst the Helvetic mountains.*"

Effects of well watering in a warm climate

It is astonishing to see, in a warm climate, the rapid growth of vegetables when they are well supplied with water. The smallest cutting of a vine will in the space of fifteen or sixteen months cover the front of an extensive edifice, or form

* Voyage dans les Alpes, 1819.

a spacious

a spacious harbour, from which the assembled family may gather in abundance of the most luxuriant grapes. In such a situation the seeds of limes, oranges, and lemons, will in four or five years produce a shady grove; and mulberry trees, when wholly stripped of their leaves for the nutriment of silk-worms, will again, in a few days, be covered thick with foliage.

Adanson, in his account of Senegal, informs us, that "when every thing green has been devoured by locusts, not a vestige of their destructive progress after a few days can be discovered."

Water decomposed both in animals and vegetables.

From the consideration of these and other facts similar to them, many distinguished chymists have delivered it as their opinion, that water is decomposed by vegetables. Mr. Chaptal says, "that the decomposition of water is *proved*, not only in vegetables, but in animals also." And for this last he quotes the authority of Rondelet.

But this not yet demonstrated.

That water, as such, enters largely into the composition of vegetables, is evident; but whether or not, and to what extent, it is decomposed, has not, as I apprehend, been yet demonstrated. In water meadows, with a plentiful supply of *running* water, vegetation proceeds even in the depth of winter, and during the severest frosts; but stagnant water is at all times unfriendly to our meadows. Any given quantity may remain upon the surface for weeks or months subject to decomposition; but instead of being in this state beneficial, it is injurious to our crops. In our water meadows we universally observe, that it is not humidity which does good, but a thick sheet of water flowing incessantly, night and day, (for a certain period) over the surface.

Probably it is a vehicle of other substances.

Hence it seems probable, that water is essential to the growth of plants, not merely as such, but as it proves a vehicle of other substances, which are their *proper food*.

Perhaps carbon their chief food.

If we may form a judgment from their analysis, *carbon* may be regarded as the chief pabulum of plants; and this we know can, in a given proportion, be conveyed to them by water. Mr. Chaptal is not only of opinion, that carbonic acid is essential to their growth, but he affirms, that the base of this acid contributes to the formation of the vegetable fibre. In support of this opinion he observes, that in fungi, which

which live in subterraneous places, this acid abounds; but by bringing them from almost perfect darkness gradually to the light, this acid disappears, and the fibres proportionably increase. This opinion is confirmed by some experiments of Mr. Senebier, in which he observes, that "plants abundantly supplied with water, which had been impregnated with carbonic acid, transpired much more oxigen, than when they were supplied with common water."

Some plants have more than others.

Some plants take more *carbon* than others into their composition; as for instance, the *agaricus quercinus, agaricus antiquus, boletus versicolor, boletus igniarius, boletus striatus, boletus perennis, clavaria hypoxylon, clavaria pistillaris*, and many others. All these contain, from the result of analysis, a quantity of carbon, nearly equal to all their other component parts. But the *lichen crispus, pinaster granulatus*, and *lycoperdon tessellatum*, contain a very small portion of carbon.

They do not retain all they receive.

Plants do not however retain all the *carbonaceous* matter they receive: they obtain more in the day when exposed to light, than they naturally require; but by the absence of light they part with this surplus, and therefore yield respirable gas only in the day-time.

Oxigen separated from them by light and hidrogen.

The separation of oxigen from plants by radiant light seems to arise from the chemical affinity between oxigen and light. For this fact we are indebted to Dr. Ingenhousz; but Humboldt was the first who ascertained, that hidrogen gas applied to plants, even when excluded from the light, occasions a separation of their accumulated oxigen.

Oxigen retained with different force, and affects the colour.

Some plants, as for instance, *tremella nostoc*, the *filices, musci*, and *algæ*, retain their oxigen weakly, and part with it readily. And it is remarked by Van Uslar, to whom I am indebted for many of these observations, that such plants as contain much oxigen, and retain it obstinately, are white; as for instance, our endive and celery, when excluded from the light; while such as contain much oxigen, and part with it easily, are generally green.

Plants require vegetable mould.

If the analysis of plants leads us to consider *carbon* as one of the most essential articles in their composition and support, no less does the existence of ages prove to us, that the principal source from which they derive their nutriment, whatever

whatever it may be, is to be sought for in *vegetable earth*, the produce of animal and vegetable substances decayed. Many plants indeed require little or no earth for their vegetation, such as the numerous *lichens* and *tragacanths*, of which genera the former were discovered by Saussnre on the highest of the Alpine granite rocks. In lower situations these form a soil for the genista, for the cistuses, and more especially for rosemary and lavender, which abound on the most elevated mountains of the Pyrenees. These again, by their decay, form vegetable earth, in which the luxuriant pine trees and the ilex grow.

Valleys.

This vegetable matter, being washed down into the vallies, helps to form and to increase their soil to a considerable depth, and to give them that fertility, which is not readily exhausted.

Soil composed of earths from the hills, & vegetable or animal matter.

When we analyse a soil, we never fail to find it composed of substances derived from a superior level. If the hills are quartsoze, calcareous, argillaceous, or magnesian, so is the soil in all the vallies which communicate with them. But with these earths in a rich soil we find a great proportion of vegetable matter, or of animal exuviæ; and as these are deficient or abound, vegetation languishes, or is exceedingly luxuriant.

Mould.

Good mould abounding with vegetable matters is commonly of a dark colour, pulverises easily, and has therefore what is called a mellow look; but when exhausted or impoverished by frequent crops, the richest soil, such as I have here described, becomes arid, of a lighter colour, compact, and comparatively barren. In a maiden soil, or where every shower of rain brings down from more elevated regions a quantity of vegetable matter, a succession of luxuriant crops may be taken incessantly, without any diminution of fertility. Thus it is in the country newly occupied by the Americans, in Kentucky, on the Ohio, and in the whole extent of territory watered by the Mississippi, or by its tributary streams. Thus also in some parts of Spain, where an extensive plain happens to receive the spoils of rich circumjacent hills, as in the well-watered vale of Orihuela, near Murcia, of which they say, "Let it rain or not rain, corn never fails in Orihuela." Indeed, so productive is wheat in this

Some will bear continual crops:

this highly-favoured district, that the farmers commonly receive 100 for 1 upon their seed.

Vegetable matter.

In my experiments, No. 10, we see, by the luxuriant growth of the cabbage and the wheat, what vegetable matter can produce. For in neither of these could any kind of nutriment be derived from the quartz sand in which they spread their roots.

Its importance in sandy soils.

The same kind of sand, in the vicinity of Barcelona, is by the assistance of a bright sun and copious irrigation rendered exceedingly productive; but then they spread upon the land all the dung they can procure, and not only station children and old women on the highways, with little baskets to collect this manure as it falls from horses or from mules, but like the farmers in the south of France they pick the leaves from the trees in autumn, and this at a considerable expense. Of such importance do they consider vegetable matter as the food of plants.

Plants affect peculiar earths:

It must be confessed, that we have frequently occasion to observe plants dependant on the nature of the earth in which they are found, and affecting each its peculiar earth, in which they grow spontaneously and thrive.

as chalk;

Thus on chalky and calcareous soils we find *thesium linophyllum, anthyllis vulneraria, asperula cynanchia, lotus corniculatus, hippocrepis comosa, poa cristata;* and three of the *sedums,* the *s. acre, s. album,* and *s. reflexum;* as on the Wiltshire downs and on the hills round Bath.

sand;

On sand we see *arenaria, rumex acetosella,* and all the sorrels; the *plantago maritima,* the *plantago coronopus,* the *onopordum acanthium,* the *sedum anglicum,* and most remarkably the *spartium scoparium.*

wet clay;

dry clay;

On clay, if wet, the *carices,* the *junci, schoenus, aira cespitosa,* and *aira cærulea, orchis latifolia,* and *orchis conopsea;* if dry, the *primula veris, orchis mas, orchis maculata,* and *poa pratensis.*

bogs;

On bogs, the *equiseta, vaccinium uliginosum, anagallis tenella, scirpus palustris, menyanthes trifoliata,* and *drosera* delight to dwell.

or the sea-shore.

On the sea-shore, and wherever the muriatic salt abounds, as near Alicant in Spain, we find *salicornia Europæa,* four

species

species of *salsola, chenopodium maritimum,* and two species of *Mesembryanthemum.*

Part of the soil decomposed.

These maritime plants appear to decompose a part of the soil in which they grow; the alkali produced by burning them, or the sal sodæ used in glass and soap, is evidently derived by them from the muriatic salt.

But earths not their food.

But when we see the *lichen parellus* fixing itself on the siliceous rock, or the *lichen immersus* affecting as it does the calcareous rock, in preference to the siliceous; whatever may influence this choice, we cannot suspect, that either of these rocks contribute by its decomposition to the nutrition of these plants; nor as I apprehend, have we reason to imagine, that either chalk, sand, or clay, is in any form the aliment of the plants.

Woollen rags very beneficial.

Woollen rags have been found of great utility as a manure, more especially for *wheat.* And in the experiments before us we may observe, that sand with rags produced a cabbage of two ounces, and four strong years of wheat. In clay with rags our cabbage weighed four ounces, and we had twelve strong years of wheat. But in what manner these rags produced effect it is difficult to say; for in January 1797 they were not visibly decayed; and in the month of September in that year they still retained their texture. The quantity we usually spread upon one acre is not more than four or five cwt.; and yet in the experience of every farmer it is found, that in the first year they nearly double the crop of wheat; and in the two succeeding years they yielded a visible increase. At present, therefore, we can merely record it as a fact, that woollen rags are highly beneficial to the land: but we cannot pretend to say by what process they contribute to the nutriment of plants.

Lime injurious.

Lime in our experiments was clearly detrimental with sand; the cabbage lived, but weighed less in January than when planted in July: the wheat had two slender stems. In clay with lime our cabbage lived till December, but never grew. The wheat had one stem, which was extremely slender, and the ear was diminutive.

Experiments seemingly dis-

These facts appear discordant with the experience of farmers in every quarter of the globe; for lime is found to be an

an excellent manure. In some parts of Wales they have scarcely any other dressing for their wheat. I well remember, that in the parish of Lansamlet, in Glamorganshire, my father, who was very attentive to agriculture, put most of his stable dung on meadow land, and used only lime for wheat. He had two lime-kilns constantly burning for his own use, and with this manure he obtained the most abundant crops; but then his land was principally a dark vegetable mould, and much of it was peat, which before it was drained had been a bog. On this land I have counted sixty grains to an ear, not picked and culled out of many others as being longer than the rest, but taken by handfuls at random. agree with experience.

In his land, lime as a dressing was particularly apt, because, as we know, it hastens the putrefactive process, and promotes the dissolution of vegetable substances, converting them quickly into vegetable mould. Attempt to reconcile them.

Now in my experiments there was no vegetable matter to be dissolved, and therefore no benefit according to chymical principles was to be expected from the lime. The trial was however made, and the received opinion as to the effect of lime is thus far confirmed.

But in my experiments the lime appears to have been deleterious. This was not from its causticity, for the plants lived; but from its action as a cement in forming a crust on the surface of the pots impervious to air. For in these pots I remarked, that after rain the water stagnated, and did not readily penetrate as in the other pots. Injurious by forming a crust on the surface.

Free *access of air* to the *roots of plants* seems to be of vast importance, and almost essential to their growth. With regard to seeds, access of air is absolutely needful to their vegetation. Hence it is that charlock *(sinapis arvensis)* will remain in the earth for centuries, if deposited below the vegetating distance, as we have occasion to observe on Salisbury-plain, where no charlock is ever seen, unless when the downs are broken up. The land is then covered with it; but till then the seeds remain as in *vacuo*, and are therefore not liable to change. Access of air to roots and seeds necessary. Seed vegetating

This deposit of seed must have happened in most remote antiquity, either when the hill country, like the low lands, after having lain in the

formed

ground for ages.

formed part of an extensive forest; or more probably when these extensive downs were subject to the plough.

Being solicitous to know whether these seeds were antediluvian, I took earth from different depths, and soon got below the stratum in which these seeds are found.

The necessity of air for the vegetation of seeds will account for effects which in agriculture are too frequently observed.

Injurious effects of a hardened surface.

If soon after wheat or barley has been sown on what is called a running sand there falls a dashing rain, the sand runs together, that is, it forms a crust, which in a great measure is impervious to air, and scarcely a grain of corn will grow; or if on clay land, during a time of drought, a garden plot is watered, and left exposed to the scorching beams of a meridian sun, the ground will *bake*, that is, the surface will be hardened, and being thus rendered impervious to air, vegetation ceases. But if the surface has been previously covered with fern leaves, as practised by skilful and attentive gardeners, no such effect will be produced. The plot may be watered and vegetation will be rapid.

Prevention.

Advantage of harrowing crops,

or hoeing them.

The admission of air, and its vast importance to the growth of plants, will account for the good effect produced by harrowing our wheat crops in spring, as lately introduced, and now universally adopted by our best farmers. The good effect produced is made apparent by the luxuriant growth of pease, beans, turnips, and cabbages, after they have been hoed; and is at present so well understood, that many agriculturists hoe their turnips twice, and their beans four times, not merely with a view to the destruction of weeds, but because they observe the benefit arising to their crops by a free admission of air into the earth. The palpable advantage of this practice has led many farmers to consider the principles on which the practice has been founded, and to try by experiments how far it can be pushed.

Fallowing rendered unnecessary.

In this pursuit, and satisfied of the benefits to be derived from loosening the surface of the ground contiguous to his crops, the Rev. Mr. Close has given up the broad-cast husbandry, keeps the hoe constantly in motion, and now finds that he has never occasion for a fallow.

But

But the most astonishing effect produced by giving free admission of air to the roots of wheat was last year exhibited by Mr. Bartley, secretary to the society of Arts at Bath. In August 1800 he sowed his wheat in rows with three feet intervals, and six inches distance from grain to grain. The proportion of seed was two quarts to an acre. The soil was a deep sandy loam, but out of condition, and filled with couch. This wheat was hoed in autumn, hoed again, and earthed up both at Christmas and spring. When it was in bloom the intervals were dug up, and it was once more earthed up. At harvest this crop yielded sixty-six bushels per acre. Such was its luxuriancy, many of the plants produced 98 perfect ears, many of which, nine inches long, contained each 100 grains. Astonishing affect of admitting air to the roots of wheat.

In the broad-cast husbandry of the hill counties of Wilts and Hants, the produce was formerly three or at most four for one, as it was in the greatest part of France. By the drill, without hoeing, the return would not be near so much; but in Mr. Bartley's crop we see more than 1000 for 1; and some grains yielded nearly ten times as much*.

I shall make but one observation more upon this subject, which is, that an orchard planted on the green sward requires double the time for its maturity as one on cultivated land, that has a more plentiful supply of air admitted to its roots. Orchards.

Thus we see that all the great agents in nature are concerned in the process of vegetation, and may be considered as the food of plants. But to determine in what manner each contributes to nutrition, must be left to the investigation of succeding generations. Conclusion.

* It must ever be with reluctance, that an exception can be taken against any argument of so able a writer as the present, especially in a matter of alleged fact. But in this instance it seems proper to remark, that the argument drawn from the reported success of Mr. Bartley should be received with caution, on account of the peculiarity of the soil. That soil being remarkably deep, fat, and productive, and within the limits of a nursery-man's garden, near a city abounding with manure, are circumstances not common to other situations. Consequently the result of any experiments made in such a spot is not to be considered as applicable to the general practice of agriculture and planting, on a large and common scale of cultivation. With the necessary allowances which the local advantage above-mentioned suggests, the consequences drawn by this gentleman may still be of importance for the consideration of our practical readers. EDITOR.

III.

Description of a Machine for Beating out Hempseed and Flaxseed, likely to be useful in Canada. By Mr. EZEKIEL CLEALL, *of West Coker**.

SIR,

Machine for thrashing hemp and flax.

I MADE a model of a machine for thrashing out hempseed and flaxseed, in the year 1803; and in the year 1805, I had a real machine made after the plan of the model, by Mr. John Wadman, carpenter and hemp merchant. The said machine has been since tried and approved by many hemp and flax merchants.

I now send the model for the inspection of the Society, and leave the event thereof to their decision. It does not injure the stalk of the hemp so much as the common mode of thrashing out the seed, and consequently leaves it much better for scaling.

I am, Sir, your humble servant,

EZEKIEL CLEALL.

West Coker, near Yeovil, Somerset,
March 22, 1806.

Certificates.

We whose names are hereunto subscribed, do certify, that we well know Mr. Ezekiel Cleall, of West Coker; that we have many times seen his machine at work, in thrashing out hempseed and flaxseed, and think it likely to be of great public utility; inasmuch as two women, whose wages and allowance never exceed one half of what are allowed to two men, will do as much work in any given time as such two men.

That the seeds thrashed by this machine are not so much bruised or injured as by the old or common way, and the hemp and flax are preserved from many injuries which they suffer from the old method.

* Trans. of Soc. of Arts, vol. XXV, p. 143. Twenty guineas were voted to Mr. Cleall for this invention.

In

Mr. Cleall's. Machine for thrashing Hemp. Mr. Bond's. Machine for breaking Hemp

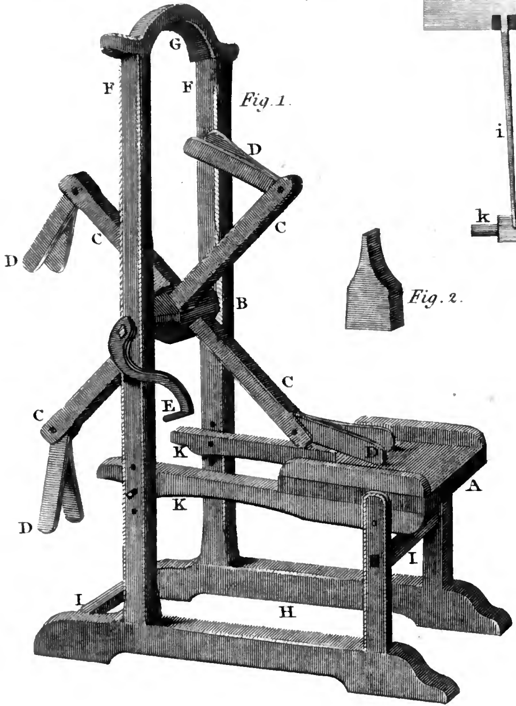


Fig. 1.

Fig. 2.

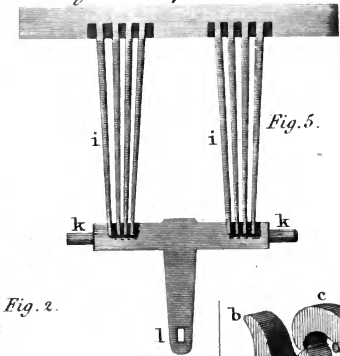


Fig. 5.

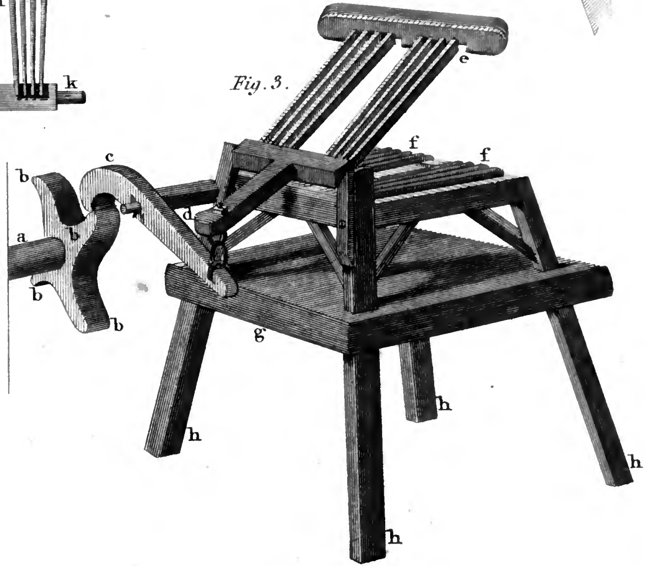


Fig. 3.

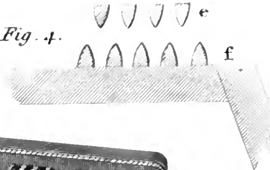


Fig. 4.

R. Roffe sc 5 Queen Ann St. East

In witness whereof, we have hereunto added our signatures.

JOHN WADMAN.
JAMES WADMAN.
JOHN BAKER.
JOHN PINNEY.
JOHN CHAFFEY.

SIR,

Machine for hemp seed.

THE machine, of which a model was sent to the Society some months ago, must be used with eight flails, two on each arm, for beating out hemp seed.

For flax seed.

When required to be used for beating out flax seed, the above eight flails must be taken out, and four beaters put in their place.

Dimensions.

The height of the machine from the floor to the top of the board on which the flax or hemp is laid, is two feet; the breadth, two feet ten inches; the length of the board, four feet four inches; the length of each of the arms, from the axis of the machine, is three feet two inches; the flails for the hemp seed, two feet two inches long; the heights of the uprights, seven feet two inches; the beaters for the flax seeds, are each one foot three inches long, and seven inches broad.

Work performed by it.

The machine will thrash, in one day, as much hemp as grows on an acre of land, and other crops in proportion; and the work is done with less than half the expense of thrashing in the usual way.

I am, Sir, your obedient servant,

EZEKIEL CLEALL.

Reference to the Engraving of Mr. Cleall's Machine for beating out Hemp Seeds and Flax Seeds. Pl. II. Fig. 1, 2.

Explanation of the plate.

Fig. 1. Represents the machine for beating out hemp seeds, in which A is the table or board on which the hemp is to be placed; B the axis in which the four arms CCCC are fixed; DDDD, eight single flails, moving upon four pins near the extremities of the four arms; these flails diverge from the pins on which they move, so that two of them

them united on each arm are nearly in the form of the letter V. E is the winch or handle by which the machine is put in motion; F F, two upright pieces of wood to sustain the axle of the machine; G, an upper cross piece, to secure the uprights firm; H H, the two bottom pieces or sills, in which the two uprights are mortised, also the two smaller uprights which support the board or table A; I I, two lower cross pieces to secure the machine firmly; K K, two levers on which the table A rests, and by which it may be raised or lowered, as thought necessary, by iron pins, at K K, passing through these levers and the two uprights.

Method of using the machine.

When the machine is used, the hemp must be laid on the table A, and moved about in different directions by the person who holds it, whilst another person turns the machine by the handle E; the flails D of the machine fall in succession on the hemp; as the axis moves round they beat out the seeds as different surfaces of the hemp are exposed on the table, and when the seeds are all beaten out from one parcel of hemp, a fresh quantity is applied upon the table.

Flax machine.

Fig. 2. Represents one of the flax beaters, which is made of a solid piece of wood, one of which is attached instead of the two flails, to every arm, when the machine is employed for beating out flax seeds, as they require more force to separate them from the flax plant.

IV.

Observations on the Culture of Hemp, and other useful Information, relative to Improvements in Canada. By WILLIAM BOND, *Esq., of Canada**.

Observations on the culture of hemp.

Culture of hemp in Canada desirable.

THE culture of hemp in Upper Canada is no doubt one of the most desirable objects with every person of discernment

* Trans. of Soc. of Arts, vol. XXV, p. 147. The silver medal was voted to Mr. Bond for this communication.

ment settled there, and more particularly so with those of this description in our mother country; and though there are so many millions of acres so well calculated to the growth of this highly valuable article, yet I do not expect much progress therein for some time, for the following reasons.

Obstacles to its introduction.

The part of the country the best calculated for the growth of hemp is so lately and in so small a degree occupied, that few have begun to use the plough, but depend upon raising a sufficiency of grain by harrowing only; in this they are not disappointed for two or three crops;—in the mean time they clear away fresh fields from the woods, many of them to a large extent, which take up so much time in fencing and dressing, that few of the farmers have been able to raise more than needful for their own families' consumption, and for the use of their neighbours; indeed they are ignorant as to the growth and management of hemp, and in general so poor, that they cannot afford to raise any thing for sale that will not bring them ready money as soon as brought to market; and grain brings such a high price in cash, that few farmers are inclined to turn their attention to any other article. Another obstacle is, there being no person or persons appointed to buy small quantities of hemp, and pay ready money for the same.

Tract of rich hemp land.

The tract of rich hemp land in Upper Canada is that part west of Yonge Street*, and north of Dundas Street †, and partly enclosed by lakes Ontario, St. Clair, Huron, and Simcoe, and to the east and north-east almost as far as Grand or Ottaway River, and to within a few miles of the south and south-east side of lake Huron. I have not failed to make annually from one to three journeys through this tract; I have crossed it in all directions with Indian guides, great part of which no white man, except myself, has ever set foot in; and I find, that the chief of the interior part consists of a rich deep black soil, which I am well convinced, when well inhabited with farmers, will become one

* A street leading from York, the seat of government, to the navigable waters of Lake Simcoe.

† Leading to the River Thames.

of the finest countries in all his Majesty's territories for the growth of hemp.

But lately begun to be cleared.

It is only about five years since this valuable tract began to be occupied at all, and though by industrious farmers, yet by such as have brought little to the country. A few cows and sheep, a *pair* of plough oxen, one or two horses, a small stock of farming tools, such as two or three axes, as many hoes and iron wedges, one or two ox chains, being the most that a new settler (generally speaking) possesses on his arrival; with these they make a shift to clear away the woods, and divide and fence the land with split timber into fields, and they are greatly encouraged to continue clearing away the forest, in consequence of the high price given for the ashes by the potash makers: this eventually will be vastly in their favour, in future, when hemp becomes the object, as it gives time for the roots and stumps of trees to rot, and their stock of horses and oxen to increase, which is essentially necessary before the farmer can expect to be successful in the growth of hemp. It is in this progressive manner, that this fine country will be settled; the nature of things demands the pursuit; and the first settlers are in a situation capable of putting the same in practice; their stock of horses and oxen are sufficiently strong to work the ground a second time over, tear up the stumps and roots, plough and pulverize the soil; and until the ground is brought to this state, it is not fit for hemp, as hemp, in its nature, depends chiefly upon a tap root, and when this root is interrupted in its progress downwards, it will throw out horizontal ones, which produce horizontal branches also, and the open spaces round the stumps of the trees admitting so much air, permits these branches to grow to such a length and strength as greatly to injure the bark or hemp of the stem. Such hemp, when it comes to the hackle, breaks off, and drags away at the knobs of the branches, so as to leave it short, and make a very great waste. Notwithstanding, if there was a sure market for as small a quantity as 50lb., there are few farmers but would try the experiment; and if one was more successful than the rest, his neighbour would endeavour to find out the reasons why it was so. Thus, step by step, the knowledge in the management

Hemp requires a soil well pulverized.

ment of hemp would be greatly extended, the farmer would generally be in possession of fresh seed, and when grain becomes less an object, he would feel no fear in turning his attention to the culture of hemp upon a large scale: and, in order to encourage the farmer, it would prove highly advantageous to take in any quantity, great or small, of sound hemp, assorted perhaps into four or five qualities, according to its length, which will vary for some years to come, for the reasons before given.

High price of labour and of grain.

The high price of labour, owing in some measure to the high price of grain, is such, that hemp, agreeable to the present regulations, is not an object with the farmer; if an addition of about a third of the present price was given, it would be an inducement for the farmers to cultivate their old fields in a more spirited manner; which bounty might be taken off again, when grain becomes less an object than it is at present, which will soon be the case in time of peace, and no doubt will affect the price of hemp in proportion in the English market.

Contrivances for diminishing labour.

In all new countries where labourers are scarce, we find many contrivances calculated for the purpose of reducing labour, more for the sake of expedition than ease; such, for instance, as the saw mill, the hoe ploughs, scythe and cradle for cutting and gathering grain, the wooden machine (drawn round by one horse) for thrashing grain, the iron shod shovel, drawn by oxen, and held by two handles, as a plough, for the purpose of levelling the roads, &c. Nor are the Americans, or other settlers in this country, fond of any work that needs violent exercise of the body; which

Disadvantage of breaking hemp in the old way.

the breaking of hemp in the old way certainly occasions, in consequence of requiring a cross motion of the arm, which makes the breakers complain of a pain about the short ribs on the side they hold the hemp; and on the opposite side a little under the shoulders, so that breaking of hemp in the old way is a great obstacle to its increased culture. To render labour, therefore, somewhat more easy and expeditious, is an object worthy the first attention, and I consider it practicable at a small expense, and have sent to the Society a model of a machine for this purpose.

Dash wheels

I have observed among the clothiers' and fullers' machinery,

erected across streams.

chinery, great power and rapid motion proceeding from what is commonly called a dash wheel, erected across a stream of rapid water, the flies or float boards of which are fixed in the octangular axis, from fifteen to twenty-five feet in length, and from three and a half in depth, each fly. I have seen many corn mills in Upper Canada, with no other water wheels than such as the above described, which save a vast expense in raising dams, &c.

Well calculated for Canada.

There are a number of streams in that part of Canada, which I have endeavoured to describe, (as to the practicability of the various ways of cultivation) that are well calculated for such wheels; and where these streams or rivers are not too wide, the axis of the wheel might be extended across so as to reach the land on each side, where I propose the breakers to be fixed to go by a tilt the same as a forge hammer. Such a simple piece of machinery would not cost more than 70 or 80 dollars, as little iron would be wanted, and timber we have for nothing; and when in motion would employ four breakers and two servers, from whom I should expect as much good work as fifteen or sixteen persons could possibly do in the old way, and that without much bodily labour.

Mills for breaking hemp.

Mills for breaking hemp, on the very same principle as that of a saw mill, as to motion only an addition of an iron crank, so as to run with two cranks instead of one, with something of a larger sweep than that of a saw mill, would be of vast utility in a neighbourhood of a large growth of hemp, and would not cost more than a common saw mill. As the brakes of the frame continue in motion the same as that of a saw mill, twenty men might be employed, who would do as much as fifty or sixty could do in the old way, and with much more ease and pleasure to themselves; and this is not the only advantage that would result from such mills; it would cause something of a social meeting, which the youth would be particularly fond of. At such meetings all the defects respecting the culture and management of hemp would be examined into, and those who raised the best would become ambitious, and try to excel each other; thus we might reasonably expect, that Upper Canada would far

Some collateral advantages.

exceed

exceed all other countries in the world for the growth of good hemp.

Reference to the Engraving of Mr. Bond's Machine for breaking Hemp. Pl. II, Fig. 3, 4, 5.

Fig. 3. *a*. Represents the axis of a water wheel, on which is fixed a trunnion of four lifters *b b b b*, each of which lifters raises in succession a lever *c*, which, by means of a chain connected with it, pulls down another lever *d*, and thereby raises the upper part of the double brake *e*. As each lifter of the trunnion passes the lever *c*, it allows the upper part of the brake to fall upon the hemp placed on the lower part of the brake *ff*; and by its weight, and teeth intersecting the teeth of the lower brake *ff*, the woody parts of the hemp plant are separated by repeated strokes from the filaments or fibres of the hemp proper for use. This completes the first operation necessary in the preparation of hemp. *g* is a table on which the woody parts of the hemp fall, and which gives security and strength to the frame; *h h h h* are the four legs or supports of the frame. Description of the machine.

Fig. 4 shows a section of the teeth of one half of the double brake abovementioned; it is betwixt the upper and lower rows of these teeth that the breaking of the hemp takes place, by the repeated rise and fall of the upper part of the brake upon it.

Fig. 5 shows the upper part of the brake, in which *i i* show the two rows of teeth, *k k* the two pins on which it is moved, *l* the part to which the chain which raises the upper part of the brake is attached. After the breaking of the hemp, it is wholly finished for use by scutching or swingling, an operation which may be either performed by the hand or machinery, and is easily executed by either mode.

The machinery for breaking hemp should be removed from the rivers previous to the beginning of the frosts.

On the breeding of rabbits.

To include the interest of the colonists and the mother country also in one and the same pursuit, is not only laud- Interests of [illegible]

able,

try should go together.

able, but most likely to succeed; especially where only a trifle of property of the individuals or of the public is wanted to set the bountifu hand of Nature to work in a country where animal subsistence and a suitable climate call for the industrious husbandman, who may in various ways be useful to himself and his country.

Warren rabbit.

In my travels through America, I have often been surprised, that no attempt has been made to introduce, for the purpose of propagation, that useful little animal, the warren rabbit, of such vast importance to the hat manufactory of England. It is chiefly owing to the fur of this animal, that the English hats are so much esteemed abroad. It is a fact well known amongst the hatters, that a hat composed of one half of rabbit wool, one sixth old coat beaver, one sixth pelt beaver, and one sixth Vigonia wool, will wear far preferable to one made of all beaver, as it will keep its shape better, feel more firm, and wear bright and black much longer.

Its fur essential to good hats.

Importance of rabbits to this country.

The value of the rabbit wool, the produce of the United Kingdom only, is not less, I will venture to say, than £250000 per annum; but the quantity is much diminished, owing to the banishment and persecution they meet with on every side, and so many small warrens taken in for grain land; in consequence of which it is time, that some protection should be afforded, if possible, to that important branch of British manufactory (in which rabbit wool is used) from suffering any inconvenience in the want of so essential an article, and the accomplishment of this grand object I conceive perfectly easy.

The warren rabbit only of value.

General Observations.—When I speak of the warren rabbit, I have to observe, that there are in England, as well as most parts of Europe, three other kinds, viz. the tame rabbit, of various colours, the fur of which is of little value, except the white; the shock rabbit, which has a long shaggy fur of little value; the bush rabbit, like those of America, which commonly sits as a hare, and the fur of each is of a rotten inferior quality.

Two sorts.

To return to the warren rabbit.—There are two sorts in respect to colour, that is, the common gray, and the silver gray, but little or no difference in respect to the strength and

and felting qualities of the fur. The nature of this animal is to burrow deep in sandy ground, and there live in families, nor will they suffer one from a neighbouring family to come amongst them without a severe contest, in which the intruders are generally glad to retire with the loss of part of their coats, unless when pursued by an enemy, when they find protection. Manners.

It is scarcely worth while for me to mention a thing so generally known, viz. that rabbits, particularly those of the warren, are the most prolific of all other four-footed animals in the world; nor do I apprehend any difficulty would attend the exporting this little quadruped with safety to any distance, provided it was kept dry, and regularly supplied with clean, sweet food, and a due regard to the cleanliness of the boxes or places of confinement. Prolific, and easily exported.

Twelve or fifteen pair of these valuable animals taken to Upper Canada, and there enclosed within a small space of ground suitable to their nature, but furnished with a few artificial burrows at the first, by way of a nursery; and spread over those now useless plains, islands, and peninsulas, so well calculated to their nature; would, I will make bold to say, the eighth year after their introduction, furnish the British market with a valuable raw material, amounting to a large sum, increasing every year with astonishing rapidity, so as to become, in a few years, one amongst the first of national objects. Would soon become highly profitable.

It may be supposed by some, that the above project is magnified beyond possibility, or even probability; but the serious attention I have paid to the subject, these many years past, as to all points for and against, leaves me no room to accuse myself of being too sanguine; for if properly managed a few years at the first, I cannot find a single thing likely to interrupt their progress.

Some idea of the astonishing increase of the rabbit may be had from the following facts:— Increase of a pair in one year.

An old doe rabbit will bring forth young nine times in one year, and from 4 to 10 each time; but to allow for casualties, state the number at 5 each litter.

In

In nine months	45
The females of the first litter will bring forth five times the proportion, of which is 2½ female's produce	62
Those of the second litter 4 times produce	50
Ditto of third ditto 3 ditto	37
Ditto of second ditto 2 ditto	25
Total in one year from one pair	219

The third female race of the old dam, and the second of the first litter, seldom breed the first year, but are early breeders in the spring following, when we might expect an increase of the whole in proportion to the first pair, if properly attended to and protected.

Hares.

It is generally allowed, that hares are not more than one fourth as prolific as rabbits, notwithstanding, agreeable to an experiment tried by Lord Ribblesdale, who enclosed a pair of hares for one year, the offspring was (as I have been credibly informed) 68: these animals, could they be exported to Upper Canada with safety, and there protected within enclosures for a few years, would soon after spread over a large extent of country: the fur is nearly as valuable as that of the rabbit.

Climate of Upper Canada.

In that part of Upper Canada within 45 degrees of north latitude, and the southern and western boundaries, the climate is nearly the same as that of England, a little hotter a few days in summer, and a little colder a few days in winter, according to Fahrenheit's thermometer, which I have paid great attention to for some years, comparing the same with the observations of the English.

Animals increase fast in America.

The increase of most animals appears much greater in proportion in America than in England, mankind not excepted. That of sheep is very apparent to those that pay attention to their breeding stock, which gives me hopes, that in a few years we shall be able to pay for our woollen cloths in wool. Finding the effect of soil and climate so salutary to sheep, &c., it may be reasonably supposed, that rabbits will answer the most sanguine expectations; as I understand the wool of the sheep retains all its nature the same as in England,

England, particularly its strength, and felting qualities among the hatters; which assures me, that rabbits' wool from those bred in Upper Canada will do the same; and there are some millions of acres, within the latitude and boundaries which I have before described, suited to the nature of the warren rabbit; nor do I apprehend that the wolves, foxes, &c. of Upper Canada will be half so destructive as the poachers in England.

The Guanaco,

or camel sheep of South America, no doubt will be a national object at some future period. This is a tame, domestic animal, very hardy, and used with much cruelty by the natives in travelling over the mountains with their burthens. It shears a fleece of wool of from 2lb. to 3lb., which is of a dusky red on the back, on the sides inclined to white, and under the belly quite white; its texture is very fine, yet strong; its felting qualities are very powerful; and it is worth, when ready for use, from five to fifteen shillings per lb. This animal would no doubt thrive, and do well in England, Upper Canada, and in particular I should suppose in New Holland. The guanaco.

The Beaver

might be propagated to great advantage in Scotland, Ireland, and the northern parts of England. It is an animal, when tamed, very familiar, and will eat bread and milk, willow sticks, elm bark, &c., and no doubt might be imported with safety; but as these two last mentioned animals are not likely to be attended to immediately, I shall say no more respecting them for the present. The beaver might be introduced into Britain & Ireland.

Pine Timber.

There are many thousands of large pine trees on the borders of the lakes, rivers, &c., in Upper Canada, which might be marked and secured for naval purposes, and which might be floated down to Montreal and Quebec with great ease, and which no doubt would be of great benefit Pines for masts.

nefit in furnishing a large supply of good masts for the navy of this empire.

I am, Gentlemen, with respect,

Your obedient servant,

WILLIAM BOND.

V.

Remarks on sundry important Uses of the Potato.*

On the use of the potato

THE potato has, though deservedly, occupied so much of the attention of different writers, and of this Society, that it may seem almost *necessary* to bring forward some new and important discoveries concerning it, if we attempt to say more on its qualities. It is not however, a singular opinion, that so important is this vegetable, and so applicable to economical uses, as human food; that it will remain for posterity fully to appreciate its positive and comparative value. But as no new and promising experiment, however imperfectly conducted, should be suffered to escape general notice, it will be acceptable to our readers to receive a general statement of certain trials made by a very respectable *British merchant*, who is also a member of the Society, with a view to ascertain the value of the potato for *sea provision* and other *stores*. His diffidence about having done justice to the subject, which he doubts of finding leisure to prosecute, prevents his allowing his name to appear as to a finished Essay of his own, for this volume; but certain statements laudably reported by him to the Society, are deemed too important to be lost, as they may lead to farther discoveries and facts. The statements then are in substance as follow:

for sea stores.

Cheap methods of preserving potatoes have not been sought after.

"The ease with which this root is prepared by boiling and for immediate consumption, either in its separate form, or mixed in bread; the little trouble there is in preserving it

* Bath Society's Papers, vol. X, p. 293.

it through the winter months; and the short period between the time of planting, and the return of the crop; have most probably been the causes, why *less pains* have been taken to find out cheap methods of preserving potatoes, as a store for future sustenance, than would otherwise have been the case.

" The large quantity of potatoes produced in the last season, and the reputed scarcity of bread corn, induced me a few weeks since to make some small experiments on the means of drying potatoes, either in substance or in flour; either for future consumption at home, or for the supply of our seamen on long voyages.

Experiments in drying them.

" The *ease* with which I found this might be done, and the probable benefit which I think may be derived to the public from a farther *pursuit* of the subject, induces me to submit to the inspection of the Society a small quantity of the flour of potato sent herewith.

This may easily be done.

" The potatoes were boiled with their skin on, dried on a kiln, and the whole ground in a steel corn mill: none of the skin has been separated by dressing.

Potato flour.

" By experiments that have been before made on fine dried flour of potatoes, it is known, that it will keep longer than the flour of wheat, without spoiling; that it is used as a substitute for sago, and makes good biscuits without admixture. And I have every reason to believe it will mix and make good bread, in a much larger proportion with wheat flour, than has hitherto been employed of the boiled root, in the common mode of using it.

It will keep longer than wheat flour.

" The expense of preparing the flour from the root in large quantities, I am not prepared to speak to. The chief labour is washing the potatoes from the mould which adheres to the eyes, particularly in those sorts, the eyes of which are much depressed. Drying them will be considerably expensive; but I think may be reduced much below what at first it will be estimated at. Grinding will not cost more than corn.

Washing the chief labour.

" From what I believe were accurate experiments, I find that one hundred pounds of washed potatoes will produce full twenty-five pounds of flour (such as the sample). The difference in weight will be very little, whether the potatoes are

Boiling not necessary,

are boiled, or only ground in an apple mill, and the juice suffered slowly to drain from them before they are dried. It might seem therefore at first view, that the boiling might be omitted; my trials however have shown me, that the colour of the flour is much fairer when boiled, and the taste more pleasant; and that the expense of boiling in steam is very little. With the greatest care even some of the starch (the most nutritive part of the root) will separate with the juice; above three pounds of fine starch (weighed after it was dried) passed off with the water from 100lbs. of potatoes.

but advantageous.

"Other persons will, I trust, ascertain such facts with more accuracy; I myself hope soon to ascertain more satisfactory particulars. In the mean time permit me to make an estimate of the probable produce of an acre of potatoes in quantity, when reduced to the state of flour.

Quantity of flour from an acre of potatoes.

"The average produce of an acre managed with care, estimated at about eighty sacks of 240lbs. each.

"According to my experiments (as before) 100lbs. of washed potatoes will produce 25lbs. of dry flour; or each sack 60lbs.; or one acre, two tons and upwards.

"I am not qualified at present to carry these calculations farther—if quantity alone be the question, I need not.

"*Note.* The potatoes used in the foregoing trials were the red apple potato.

Peeling.

"The steel mill has not ground this flour so fine as I believe a stone mill would have done. Some of these had their skins stripped off after boiling. Should an expeditious method be found of stripping off the skins, it will perhaps be less troublesome than washing so carefully as must otherwise be practised."

After giving a numerical account of the samples of flour of potato prepared for exhibition; this gentleman gives also samples of bread and biscuit made from different sorts of potato flour, mixed with different proportions of wheat flour of different degrees of finenes; but these would be unintelligible in this place, in the absence of such samples.

Maufacture of the flour.

"The potato flour used in the bread and biscuit is made of the *whole* of the potato, washed, steamed, bruised slightly

slightly after steaming, dried on a malt kiln, and ground in a common corn mill, no alteration whatever having been made in the set of the stones, from what they were as used for grinding wheat; it may reasonably be supposed however, that a miller, accustomed to grind this article, would make better work and finer flour.

"Nothing was taken from the flour except some large pieces that were not ground, and a little large bran in the proportion of the sample sent herewith.

The potatoes should be dried without delay after boiling, & not over dried.

"The potatoes of which this flour was made were certainly over dried; and having lain in a heap after steaming upwards of two days before they were put upon the kiln, some degree of fermentation had begun to take place, but which was thought so little as to have been perfectly corrected by the drying. In the bread, however, it is certainly distinguishable. The baker considers, that it is from this cause that the bread is not so light as it otherwise would have been. It rose well in the oven, but fell when the door was opened. He thinks that when mixed with the flour of dry wheat, the potato-meal will have exactly the same effect as the mixture of a certain portion of cone wheat flour, and that it will answer as well in about the same proportion. He has no doubt, but that even with this flour he shall succeed better in the second attempt. With potato meal well made, he believes that bread of the best quality may be produced.

Similar to cone wheat flour.

Precautions.

"The chief precautions necessary in making potato flour seem to be, to prevent any fermentation taking place in the boiled potatoes, previously to their being dried, and to avoid giving them too great a heat in drying. With this view it seems advisable to construct the apparatus for preparing it, so as that the steaming tubs and kiln should be heated by the same fire, without loss of time or labour; the potatoes may then be immediately removed from the steam to the kiln, and means should be used to regulate the heat of the kiln, so that it should not much exceed 90°.

Peeling not necessary.

"For the common purposes of bread, it seems evident, from the samples, that taking off the rind or skin is by no means necessary; to wash the potatoes carefully before boiling seems, therefore, the only precaution required.

"From

More potatoes may be used in making bread this way than raw or boiled.

"From experiments as before stated, the produce of dry meal is to the raw potato, as 26 or 27 to 100, but let it be estimated at 25 or 1 qr. of the whole. The greatest quantity of *raw* potatoes said to be used as a mixture with wheat flour in bread is one third; not much above the same quantity of boiled potato has usually been employed. The proportion of flour in boiled potato exceeds that in raw potato by about 1 qr. As a rough ground for calculation, we may take 33 per cent as the proportion of flour in any given quantity of boiled potato.

"The proportion therefore which the potato meal makes of the whole mixture in this bread, above that in which one third raw potato has been used, is four times: that is, the actual quantity of potato flour in this bread is as great, as if 24lbs. of raw potato had been mixed with 12lbs. of wheaten flour; and compared with *boiled* potatoes, it is as great as if 18lbs. of potato had been mixed with 12lbs. of wheat flour."

Practical application.

From the foregoing statements, it is not presumed that much farther information is imparted, than may have been gathered from some former accounts of bread making from a mixture of such flours, except as to the mode of preparing the potato flour. Neither is it at present supposed, that for common use, when corn is not dear, the potato will supersede the use of neat wheaten flour for family bread. But in very dear times, when it may be used in some places to great advantage, the most economical mode of doing it is important; and the process of steaming, kiln drying, grinding, and dressing, seems excellent. If *equal* quantities of wheat and potato flour are found to make very good bread, and the potato to have the effect of *cone flour* in the mixture; this may be set down as a sufficient regulation, and a valuable fact.

Potato flour almost imperishable.

But what is of great consequence to be known and fully noticed is, that the flour of the potatoes so prepared, if barrelled up, and kept in any common dry place, will retain its virtues longer either on land or at sea, than the other sort of flour made from grain: in short, from frequent appearances and well attested facts, the flour of this vegetable, prepared

prepared as aforesaid, seems to possess the singular quality of being almost *imperishable.* In addition to this quality, the power of preserving potatoes in barrels, after being kiln dried, either when whole or cut into parts, for the use of the table in long voyages, is very important; and it is found, that, after being so preserved, they are capable of being again boiled soft, and served up as a vegetable at table, retaining much of their original flavour, consistence, and other qualities.

EDITOR.

☞ For two valuable papers on the fecula of potatoes, and its uses, by Mr. W. Skrimshire, jun., see Journal, vol. XXI, p. 71 and 182.

VI.

On the Dissimilarity between the Creatures of the present and former World, and on the Fossil Alcyonia. From Parkinson's Organic Remains.

SOME of the extraordinary circumstances which have arrested our attention, whilst examining into the nature of fossil corals, now demand a few general remarks. You cannot but have observed how completely I was foiled, in my attempt to preserve a parallel between the fossil corals which I have particularised, and the several corals which are enumerated in the *Systema Naturæ* of Linnæus. Indeed, so little could this parallel be preserved, so little agreement could be traced between the recent and the fossil corals, that I find myself under the necessity of acknowledging, that I am not certain of the existence of the recent analogue of any really mineralized coral.

Great dissimilarity between recent and fossil corals.

This dissimilarity between the creatures of this and the creatures of the former world, is a circumstance which appears to be so inexplicable, that I can only admit it, without attempting to account for it. It however furnishes us, I think, with a strong argument against that theory, which supposes the changes which this planet has undergone are all attributable to the constant, regular, and gradual processes

This inexplicable.

The present state of our world not the effect of regular workings of nature:

cesses of nature, which have been acting from an indefinite period of time, aided by the occasional heavings of strata, effected by subterraneous heat. By this system—by the gradual interchange of situation between land and water, we might account for the mountains of fossil coral which are found at considerable distances from the sea, were it not that so little agreement is observable between the fossil and the recent coral. Had the coral of the mountain and the coral of the sea been constantly the same, it would, indeed, have furnished a powerful evidence of the gradual change of relative place in the strata, which were once covered by the ocean, but which are now thousands of feet above its surface: the gradual receding of the sea would have sufficed for the explanation.

But how, according to this theory, shall we explain the disagreement between the coral of the mountain and the coral of the sea? I see no explanation which can be thus obtained: every thing being supposed to have proceeded in its regular course, the animals of the first creation must then have exactly resembled those of the present hour. Some vast change, of powerful and even universal influence, must be sought for, to explain this wonderful circumstance: and such, doubtless, can only be found in the deſtruction of a former world. Thus, indeed, we shall be enabled to account for the existence of various animals, in a mineral state, whose analogues are unknown; but it must be admitted, that even this circumstance is not sufficient to account for the existence of animals at the present period, of which no traces can be found in the ruins of that former world.

but of some great catastrophe.

Fossils of animal origin resembling vegetables.

We now arrive at the examination of that class of bodies, of which it was remarked, in the former volume, that although they were decidedly animal substances of marine origin, yet, from the resemblance which they bore to terrestrial fruits, their animal origin had been doubted, and they had been considered as petrified oranges, figs, funguses, nutmegs, &c.

There is no substance which has attracted our attention, during the prosecution of these inquiries, which can yield so many subjects for investigation as these bodies. For whether

whether we consider the peculiar forms with which they are endowed, the original modes of their exiſtence, or the extraordinary changes which they have undergone, a variety of subjects of inquiry, of the most curious nature, will necessarily arise.

That many terrestrial fruits and seed-vessels, containing the ligneous matter, have been found in a petrified state, has been already shewn: of these, of course, it is not intended here to speak. But substances have been repeatedly met with, the general appearances of which have so much accorded with those of some terrestrial fruits, as to have led several learned and ingenious men to place them among these substances. Thus Volkmann was deceived, and figured and described one of these bodies as *nux moschata fructu rotundo*, Casp. Bauhin *. Scheuchzer, on the authority of Volkmann, adopted the same figure and description. Nor will this errour be considered as without excuse, when the great resemblance of many of these substances to terrestrial fruits is shewn. Indeed, I much suspect that, after all the circumstances have been examined, some persons will be found who will not be readily disposed to consider subſtances, bearing such appearances, as subjects of the animal kingdom. The propriety however of doing this will perhaps appear, when other bodies will be shewn passing, through almoſt insensible gradations, from these bodies, which so closely approximate, in their general appearances, to the subjects of the vegetable kingdom, up to others, whose characters are sufficiently marked, to leave no doubt whatever in the mind as to their animal origin.

Many have been deceived by the closeness of the resemblance.

No one I believe has been more industrious, or more successful in their inquiries, respecting these bodies than M. Guettard, as appears by his very ingenious Essay, *Sur quelques Corps Fossiles peu connus*, in the Memoirs of the Academy of Sciences at Paris for the year 1757. M. Guettard observes, that at Verest, near Tours and Saumur, and at Montrichard, in Touraine, there are found, at some depth in the earth, numerous bodies, which from their very close resemblance, in figure, to figs, pears, oranges,

Guettard very successful in his inquiries into them.

* Silesiæ Subterraneæ. Tab. XXII. Fig. 6.

and

and other fruits, are there considered as fruits, which, having fallen from their trees, have been buried in the earth, where they have undergone the process of petrifaction. These bodies, it appears, not only differ very much from each other, in their forms, but also in their ſtructure: and in Mons. Guettard's judgment are divisible into two kinds; those which possess somewhat of a globular form, and those which are conical or funnel-formed.

Two kinds of them.

The former, he observes, may be divided into the body or globular part, and the pedicle or elongated part. In the centre of the superior part of the body is a circular opening, which, in some of the specimens, is closed by extraneous matter, derived from the matrix in which they lie. This opening, which is larger in its upper part than it is downwards, is continued almost to the pedicle, and in some specimens appears even to penetrate it. This is however very difficultly ascertained, since the opening is in general loaded with the extraneous matter. From the circumference of this opening lines may be traced, which not only pass over the whole of the spherical part, and inosculating, are continued to the elongated part, where they form striæ more or less plain; but they are also found to penetrate into the substance, both of the body and of the pedicle. These bodies have, in general, but one of these openings, but some have more; and Mons. Guettard found one with three distinct openings. In this specimen, the lines or striæ just mentioned were seen to collect around the circumference of each of the openings, and after inosculating, to pass into the pedicle, in nearly the same manner as in the others.

The pedicle varies greatly.

A great disproportion, it appears, is frequently observable between the size of the globular part of these bodies, and their pedicle; sometimes the pedicle appearing very large, and sometimes very small in proportion to the body: this difference is however frequently the consequence of the pedicle having been broken off; a circumſtance which indeed so often occurs, that a perfect specimen is very rarely to be met with: numerous fragments of the pedicles being dispersed about in the places where these bodies are found.

The

The pedicles are in general of a conical form, and not unfrequently flattened.

By grinding the globular part as well as the pedicle on a stone, he discovered that their texture appeared to be similar, and that by the frequent ramifications of the fibres, of which their substance was composed, a net work was formed, not much unlike the parenchyma of vegetables. We therefore perceive that a loose resemblance, sufficient to excuse the vulgar opinion of their origin, is observable between these bodies and the terrestrial fruits. These bodies, like fruits, appear to have been formed chiefly of a parenchymatous substance; their pedicle seems to answer to the ſtalk; whilst the opening on their superior part agrees with what is termed the eye of fruits. But a little attention shews that, unlike to the parenchyma of fruits, which is formed of vessels terminating in minute points, the substance of these bodies is formed of a species of net-work, which, as M. Guettard observes, if all the matter contained within the meshes could be removed, would resemble a skain of thread, of which one part, answering to the pedicle, is pinched together, and the other, answering to the body, is spread out without being cut. Again, the eye, in fruits, is not pervious, as is that part which answers to it in these fossils; nor does the pedicle at all agree with the stalk of fruits, either in proportionate size, or in figure.

Texture of both parts similar.

Its difference from that of fruits.

Scheuchzer describing a fossil of this kind refers it to the *alcyonium stupposum Imperati* *; but of the identity of these substances Mons. Guettard, with much propriety, doubts; although he allows that the external form, and particularly the opening in the upper part, might readily lead to this supposition, This doubt arose in the mind of M. Guettard, from comparing the ſtructure of one of the alcyonium stupposum of Imperatus with the description of its structure as given by John Bauhin and by Count Marsilli; the result of his comparison being, that both the descriptions were in some respects erroneous. Taught by careful examination, he states it to be composed of fibres, more or less fine, intersecting each other, without order or

Fossil supposed to be a sea-fig.

* Lithograp. Helvet. P. 15.

regularity,

regularity, and anastomosing together by their ramifications, by which they form irregular meshes of various figures and quite empty. By this contexture a spongy mass is formed, which is covered by a thin pellicle, constituted in the same manner, excepting that the texture is more close and compact, and extended into a membrane-like substance, which may be detached and easily raised from the body, and which, when examined by a lens, appears to be a mass of fine fibres forming very small meshes, similar to the large ones of which the body is composed. The foot stalk, which spreads out and is a species of basement by which the fig is attached to the body on which it grows, does not seem to differ from the general substance in its conformation. Hence M. Guettard concludes the sea-fig to be merely a sponge, differing from other sponges only in form, and possessing like them the property of imbibing water and losing it by compression.

The sea-fig a sponge.

Difference between this and the fossil.

On comparing the structure of the sea-figs with that of these fossils, M. Guettard points out differences which are undoubtedly very essential. In the pedicles of the fossils, he observes that circular points may be seen, which will be found to be continued into the spherical part of these bodies; so that by different transverse sections they may be traced, passing on like so many veſſels, from the pedicle into the substance, and even on to the surface of the fossil: whereas, in the sea-fig, the fibres have no such regularity of disposition, nor are they thus continued like tubes from the pedicle into the subſtance of the fig.

Fungites, or supposed petrified mushrooms.

M. Guettard next describes the other kind of fossil, which belongs to the class of fungites, and which, like the ficoid fossils just treated of, are open at their superior and wider part, and in general possess somewhat of a conical form: and from their varying in length, width, and size, frequently bear a resemblance to cups, glasses, funnels, cones, &c., whilst others are longer, cylindrical, and even fusiform. This variety of figure is frequently dependant on the circumstances of the fractures which they have suffered; these fossils, like the former, being rarely found in a perfect state. M. Guettard appears to have been entirely foiled in the attempt to discover any recent zoophyte, which might be considered as bearing any analogy with these fossils. He

He first was disposed to consider them as being similar to the *spongia elegans* of Clusius, or the *spongia dura* of Sloane, but this opinion he found reason to relinquish, and was then induced to believe that they bore a nearer resemblance, in their general characters, to some species of madrepores than to any of the sponges. In several of these fossils he discovered an outer layer, which appeared to differ from the general substance of the fossil; and his opinion, he thought, derived support from this circumstance, for, on examining the interior lamina of these fossils, he conceived that it much resembled the hard smooth part which forms the corresponding parts in madrepores, &c. Madrepores and corals, he observes, are covered by a substance which has been distinguished as their cortical part, and immediately beneath this, there is a smooth substance of very close and compact texture, in which there are no striæ nor traces of any fibres. With this latter substance, he thinks, the external layer of these fossils exactly agrees: and he is confirmed in the supposition that it originally belonged to them, and was not derived from the matrix in which they lay, by observing that, in one specimen, several little flat shells of oysters were adhering to this surface.

Single starred corals of the Baltic.

Nothing, he thinks, in the fossil kingdom approaches so near to these fossils, as the single-starred corals of the Baltic, described by Fougt. The only difference, M. Guettard remarks, is that the corals described by Fougt have ftriæ which extend from the centre of the coral to the edge, in such a manner as to form a star. This difference is however sufficient to remove all idea of similarity between the two bodies; since, as we have already seen, the star constitutes the genus Madrepora, to which those corals belong, whilst in the fossil bodies now under consideration, there exist none of the characters which mark any of the species of zoophytes, which we have hitherto examined.

Many fossils apparently of unknown genera.

Many of these fossil bodies, it will be seen, differ so much from any known recent zoophyte, that were it not that vaft numbers of these muft be concealed from us, in the numerous recesses of the ocean, they would be concluded to possess not the leaft resemblance with any animal substance now exifting: indeed, so considerable is that difference,

difference, that some substances will be placed before you, which, not only cannot be referred to any particular known species, but which would almost authorize the formation of new genera for their reception.

We shall proceed, however, as nearly as possible, according to the generally accepted systematic classification; and shall derive what aid can be obtained, from the examinations which have been made of living substances apparently of a similar nature. It is intended, therefore, to endeavour to comprise, under the genus *alcyonium* or *spongia*, the substances so accurately inquired into by M. Guettard, as well as several others which have not been spoken of by him, but are evidently of the same kind.

Difficult to distinguish alcyonia from sponges in the recent state.

With respect to the classification of these bodies, a difficulty almoſt insuperable presents itself; since the characteristic marks by which the substances belonging to these two genera are distinguished, in a recent state, are frequently not to be discovered after they have sustained the change of petrifaction. Previously, however, to proceeding further in an inquiry on this subject, it will be proper to consider the nature of both alcyonium and of sponge, and to ascertain what are the distinctive characters of each.

Characters of the alcyonia.

The alcyonium is an animal which assumes a vegetable form, and which is either of a fleshy, gelatinous, spongy, or leathery substance, having an outward skin full of cells, with openings possessed by oviparous tentaculated hydra: the whole substance being fixed to some other body by a seeming trunk or root.

Count Marsilli, who carefully examined not only the physical, but the chemical properties of these bodies, observes that they are all surrounded by a porous leather-like bark; and that the interior substance is, in some, a jelly-like matter, and in others, a mass of light ash coloured acicular spines, which prick the hands on being handled, in the same manner as do the spines of the plant called the Indian fig.

More minutely examined by Donati.

Donati, in his Essay on the Natural History of the Adriatic Sea, has made, in some respects, a more minute examination of the structure of two different species of alcy-

onia

onia than even that of Count Marsilli, and was able to ascertain by the aid of a magnifying glass, the peculiar forms assumed by the spines of which these animals are in a great measure composed. Of these we shall soon have occasion to speak more particularly.

The forms in which these animals exist are very numerous; this depending not merely on the number of species, but on the different irregular forms which the same species under different circumstances may assume. Thus Marsilli observes the same alcyonium, which sometimes grows flat, and thus covers large pieces of rocks, is at other times found in a rounded form. Exist in various forms.

From the different colours as well as forms which some of the species of these substances possess, they have obtained names expressive of their resemblance to certain fruits. Thus the *alcyonium lyncurium*, being of a globose form, of a fibrous internal structure, of a tubercular surface, and of a yellow colour, has been termed the sea-orange: the *a. bursa* being of a sub-globose form, of a pulpy substance, and of a green colour, has been termed the green sea-orange or sea-apple: the *a. cydonium*, which is of a roundish form, and of a yellow colour, has been distinguished as the sea-quince: and the *a. ficus*, from a very close resemblance to the fig in its form, has been called the sea-fig. Named from their resemblance to fruits.

The sponge is a fixed, flexible animal, very torpid, varying in its figure, and composed either of recticulated fibres, or masses of small spiculæ interwoven together, which are clothed with a living gelatinous flesh, full of small mouths or holes on its surface, by which it sucks in and throws out the water. Characters of sponges.

The vitality of sponges had been suspected by the ancients, even in the time of Aristotle; they having perceived a particular motion in their substance, as if from shrinking, when they tore them off the rocks. This opinion of their possessing a degree of animal life was also entertained in the time of Pliny. Count Marsilli * confirmed this opinion by observing, on their being taken out of the sea, Their animal nature suspected by the ancients, and confirmed by the moderns.

* Histoire Physique de la Mer. p. 53.

sea, a systolic and diastolic motion, in certain little round holes, which lasted until the water they had contained was quite dissipated. Mons. Peysonell supposed sponges to have been formed by certain worms, which inhabited the labyrinthean windings of the sponge; and believed, that whatever life was found in these substances, existed in these worms, and not in the substance of the sponge, which he was convinced, was an inanimate body. This point was, however, determined by Mr. Ellis, who, in a letter to Dr. Solander*, relates the observations which he had made; by which he ascertained, that these worms, which he found in the sponge in great numbers, were a very small kind of *nereis*, or sea scolopendra; and that they were not the fabricators of the sponge, but had pierced their way into its soft substance, and made it only their place of retreat and security. Upon examining, in sea water, a variety of the crumb of bread sponge, the tops of which were full of tubular cavities or papillæ, he could plainly observe these little tubes to receive and pass the water to and fro; so that he inferred, that the sponge is an animal *sui generis*, whose mouths are so many holes or ends of branched tubes, opening on its surface; with these, he supposes, it receives its nourishment, and discharges, like the polypes, its excrements.

Worms in them adventitious.

Mr. Ellis also discovered, that the texture is very different in different species of sponge: some being composed wholly of interwoven reticulated fibres, whilſt others are composed of little masses of ſtraight fibres of different sizes, from the moſt minute spiculæ to ſtrong elastic shining spines, like small needles of one third of an inch long; beside these, he observes, there is an intermediate sort, between the reticulated and the finer fasciculated kinds, which seem to partake of both sorts.

Texture of sponges different.

In the substances considered as alcyonia by Donati, as well as in some of those which have been described by Count Marsilli, similar large bundles of elastic fibres like needles were diſcovered. Theſe had been reckoned alcyonia by moſt authors, but in Mr. Ellis's opinion they ſhould not be

Distinction between alcyonia and sponges.

* Phil. Trans. vol. LV. p. 280.

be fo reckoned, since neither Donati nor Marsilli mentions any polype suckers extending out of their pores; he considering the exiftence of thefe as the distinguishing character of the genus alcyonium, as much as the pores without the polypes in these elastic fibrous bodies is the character of the sponges *.

It is evident that these needle-like spiculæ cannot be considered as belonging to the genus spongia only; since among the alcyonia some are admitted to be formed of a spongy substance, into the composition of which these spicules may of course be expected to enter: on the presence or absence therefore of polypes in the cells of the substance must alone depend the necessary distinction.

Most difficult in the fossil state.

But when the difficulty of distinguishing between the alcyonia and the sponges, even in a recent state, is considered, the oryctologist will easily find an excuse for his inability, to make a similar diftinction between these substances, after they have undergone the lapidifying process: when their pores have become filled; and their colour and their fubftance, and, in fact, their whole nature has been changed. Indeed, the assumed generic difference between the alcyonia and sponges is such as muft be entirely loft in moft of these substances which have undergone the change of petrifaction. Whether the pores, which are discoverable in a fossil, were the dwelling of the polypous hydræ or not, can no longer be ascertained; since their radiation, which is supposed to characterize the openings in which these minute animals exist, and which is frequently so faint in the recent alcyonium as hardly to be detected, is very likely, in the fossil substance, to be still more difficult to be made out. Indeed, from this indistinctness of the radiation, much difficulty appears to have arisen in making the necessary distinction between even the recent sponges and alcyonia; the graduation from the perfectly radiated opening of the alcyonium, to the plain opening of the sponge, being so gradual and imperceptible, as to render it a difficult task, even where the substances are in a recent state, to draw the line where alcyonium ceases and sponge begins.

* The Natural History of Zoophytes, &c. p. 183.

But

Farther difficulty from their possessing other characters, and differing from all known species.

But here is not the whole of the difficulty: several of the fossils, which will be presently described, possess some of the characters of acidia and actinia, with those of the sponge or alcyonium; thereby rendering their distinct and correct classification almoſt hopeless. Hence, although I shall in general speak of these bodies as alcyonia; I am aware, that when their histories have been elucidated by the inspection of more illuſtrative specimens, several of them may claim other designations.

The consideration of another circumſtance leads to the necessity of giving up every idea of distinguishing the alcyonia from the sponges, whilst in a mineralized state. Among the fossil zoophytes which claim a situation under one or other of these genera, by far the greater number are such as are so totally different from any known species of either alcyonium or sponge, as to render it almost impossible to determine under which genus they ought to be placed. Under these circumstances, you must perceive that the attempt to separate these fossils, by specific distinctions, at present, would be hopeless: it can only be effected when, by additional observations, their nature and forms are more perfectly known.

When it is recollected what very considerable variations in form, are found to take place in the recent individuals, of the several species into which these subſtances are divided; and when it is considered, that whilst passing into a mineralized state, their figure and appearance may be also much changed, it may be suspected that hardly any opportunity of fair comparison could be found, between the recent and fossil alcyonia.

Their change of form, when converted to stone, wonderfully little.

This however is very far from being the case; and indeed when we reflect on the transmutation which has taken place; that a soft, gelatinous, or spongy substance, has become a hard and ponderous stone, we cannot but be affected with a high degree of astonishment; especially on perceiving, that this great and extraordinary change of substance has been accompanied by so little change of form. In consequence of this I trust I shall be able to place before you many bodies, even in a silicified state, which will immediately appear to have been animals of this description,

belonging

belonging to a former world. So great indeed will be the variety of these bodies, and so perfectly well preserved will they appear, as to render it necessary for me to say a few words, respecting the state of preservation in which they are found.

This is rendered necessary; since the comparatively frequent appearance of these bodies, in a fossil state, appears to contradict a position laid down in the former volume, whilst speaking of fruits, that substances possessing a pulpy consistence were not likely to be found in a fossil state; since their decomposition would most probably take place with too much rapidity, to allow of that change being effected, on which their mineralization would depend. But a peculiarity of structure exists in these animals, which exempts them from the influence of this law. It appears, as we have seen from the observations of Marsilli and Donati, that these animals have blended, with their gelatinous and carneous substance, innumerable minute spiculæ, which may be considered as the bones of the animal. These manifest themselves by the prickling sensation they occasion, on being handled, which has obtained for some of these animals the name of the sea nettle. That these spiculæ, formed of a hard and durable matter, may, in some, and especially that the spongy fibres and coriaceous covering may, in others, keep up the form of the animal, for a sufficient time to admit of the petrifactive process being accomplished, seems to be not improbable; and indeed appears to afford a satisfactory mode of explaining this curious fact.

Attempt to account for this.

That the bodies now about to be more particularly described are the remains of animals of a former world, seems to require no stronger proof, than the circumstance of these inhabitants of the sea being found in their changed state, in mountains much elevated above the level of the sea, and at a considerable distance from the situations which it now possesses. Whilst treating of the fossil corals, many were pointed out, whose recent analogues were positively not as yet known, and which were therefore conjectured to be the remains of certain species which might be now extinct. Any opinion of this kind with respect to these animals appears to

They must have belonged to a former world.

to be hardly admissible; since from the innumerable recesses in which they lurk, and still more from the comparatively small degree of eagerness with which they have been sought, we are totally unable to form any conjecture, as to the number of those which may have hitherto entirely escaped observation. Analogy indeed may lead us to conclude, that by far the greater part of these fossil bodies are actually the remains of extinct species; but where evidence of a stronger kind cannot be also obtained, the fact must be considered as undetermined.

Fossil alcyonia described.

Having made these few prefatory remarks, I shall now proceed to a more particular examination of such fossils of this description, in my possession, as are most illustrative of the history of these extraordinary animals.

Ramified.

Those which are of a ramified form seem to be most rarely found in a mineralized state. The specimen however which is figured, Plate VII, fig. 12*, and which was found in Berkshire, is undoubtedly the fossil remains of one of these species; although it is impossible to say to what particular ramified species it belongs, or whether indeed it is at all referable to any known species.

Silex & chalk.

An examination of the substance of this fossil, now a mixture of silex and carbonate of lime, affords us internal evidence of its origin; since its texture is such, as I have found almost constantly to characterise the fossil remains of any individual of this genus, which had been composed of a sponge-like substance. This substance has evidently, like sponge, been of a reticular texture; but the disposition of the meshes, if so they may be called, is in the spongy alcyonium much more uniform and determinate than in ordinary sponge, and though not to be described in words, the texture is so peculiar and characteristic, as directly to be known by those, who have been in the habit of examining these and similar substances, by the aid of magnifying glasses.

Digitated.

The fossil represented Plate VII, fig. 6, and which is also from Berkshire, appears to bear a tolerably close resem-

* The references here and elsewhere are to the figures of the original work.

blance

blance to *alcyonium digitatum* of Linnæus; or the *dead man's hand*, or *dead man's toes* of Ellis. Its texture evidently appears to be of that kind, being finely reticulated, which would correspond with the carneous spongy substance, of which the recent zoophyte is formed. Its surface also, thickly beset with minute openings, bearing somewhat of a stellated appearance to the naked eye, serves to confirm the resemblance. This fossil is now a carbonate of lime moderately hard, but friable.

Chalk.

Priapolithi.

In the elegant work of Mr. Knorr, Mr. Walsh describes several fossil elongated alcyonia, by the silly term which the ancients had adopted, of *priapolithi*. One of these from Touraine is figured, Plate VII, fig. 1. It had at its superior termination that opening, observable in many of these animals, which served for the reception of the sea-water, from which, it is probable, they derived their support.

A retiform texture of silex,

On rubbing down this substance on a sandstone, at this termination, for the purpose of examining its structure, its hardness and the partial polish it obtained, proved, that it had suffered an impregnation with silica: and an examination of this surface with a lens plainly showed, that the flinty part was regularly distributed in continuous meandering lines, bearing the peculiar and characteristic form of the spongy part of alcyonia, whilst the intervening spaces appeared to be filled by a softer substance, a carbonate of lime. The substance was therefore partly immersed in dilute muriatic acid, by which the calcareous part was speedily removed, with effervescence, and the siliceous part left, possessing the fine retiform texture of the spongy alcyonium, surrounding the central opening already mentioned, as may be seen in the upper part of the figure.

and the interstices filled with chalk.

The fossil represented Plate VII, fig. 9, approaches the nearest, in its general form and appearance, to the *alcyonium cydonium* Linnæi, the *alcyonium primum* of Discorides, or rather to the representation of this animal as given by Donati. It must however be, I believe, considered, as differing from any known animal of this genus.

This fossil is of a roundish form, rendered unequal by shallow depressions about the width of a finger, which pass

from

from the superior to the inferior part of the fossil, and are separated from each other by tuberculated ridges. At the upper part has been a circular opening more than half an inch in diameter; and, at the lower part, is a rugged spot as though the pedicle had been here separated: a circumstance indeed which renders its affinity to the alcyonium described by Donati rather more doubtful. The substance of this fossil appears to be a limestone, which, probably from some tinge of iron, has obtained a reddish brown colour. It is not of a very close texture, apparently from the superadded calcareous matter not having accurately filled all the interstices between the fibres. Hence numerous small openings are, even in its present state, observable on its surface, which on close inspection are seen to be such as would result from a loose or spongy texture.

Limestone tinged with iron.

Spines mentioned by Donati.

Whilst treating of the alcyonium, of the species to which this seems to approach, Donati particularly describes and delineates the curiously formed spiculæ, which constitute a part of its substance. The body, as well as the cortical part, he remarks, is formed of two substances: the one of which is fleshy, and the other osseous. The latter, he adds, is formed into spines; which, near the cortical part, are in great number, and closely intermingled; being about the length of two lines, and even longer. They are either of a fusiform figure, or are finely pointed at one end, and then gradually enlarge towards the middle: then, diminishing as they lengthen, they divide into three sharp conical points, around which are fixed numerous minute globular bodies, which are chiefly found in the cortical part.

Strictures on Donati by Plancus.

A very strict examination, with a lens, of the surface of numerous fossil alcyonia, did not however discover any appearance of similar spines, and almost induced me to a ready concurrence with Plancus, who relates, that he has dissected various bodies of this kind, and has seen the osseous fibres disposed in a radiated form; but as to the wonderful bark, the structure of which is so floridly described by Donati, he says, I have not seen it, and observes that the same thing has happened to him, with respect to the greater part of the figures in Donati's book, which, he says, are embellishments of the designer, drawn by the rule and compass,

compass, rather than in agreement with the truth and simplicity of nature*.

Being in possession of another specimen of this kind, formed of a much harder and closer stone, and which from its appearance I supposed to be invested with its cortical part, I resolved to sacrifice it to a more rigorous search for the spines described by Donati, concluding that, since all agreed as to their differing in their bony hardness from the other parts of this animal, I should at least discover some traces of them, although I might not be able to make out their form.

A specimen examined

This fossil was therefore subjected to the only modes of dissection which I could employ with substances possessing a stony hardness. A polished section of it was obtained on different parts of it, and at different depths; by which the peculiar spongeous structure, already noticed as belonging to these bodies, was perceived; but no appearance of spines could be detected.

by cutting,

The specimen was then immersed in dilute muriatic acid, and examined at different periods, to ascertain whether the new surfaces thus obtained displayed any particular appearance. After rather more than a quarter of an inch of its substance was thus removed, I was pleased to find, with a lens of moderate power, several cruciform spines, formed, as it were, by two fusiform bodies, not an eighth of an inch in length, crossing each other at right angles, and terminating at each end in a very sharp point.

and digestion in muriatic acid,

which exhibited the spines.

When these bodies were first discovered, the specimen was still wet with the water, with which the acid had been removed. In this state they possessed a considerable degree of transparency, which they rapidly lost, as the water evaporated: so that when dry, they were completely opaque, and of a chalky whiteness. From their possessing this hydrophanous quality, and from their having withstood the action of the muriatic acid, there appears to be the greatest reason for supposing, that these bodies, which were originally the spines of the animal, are now formed of an hy-

These an hydrophanous chalcedony

imbedded in chalk.

* De Conchis minus notis, App. II, page 115.

drophanous chalcedony, and imbedded in a matrix of carbonate of lime, which has pervaded or has supplied the place of the soft spongeous part. This and the preceding fossil alcyonia are from Switzerland.

Alcyonium resembling the sea-fig

Alcyonium ficus Linn. accurately depicted in the *Metallotheca* of Mercatus * as *Alcyonium quintum antiquorum*, and particularly described by Marsilli as *Figue de substance d'eponge & d'alcion* †, resembles much, in form, the brown silicious fossil, Plate IX, fig. 4. The recent alcyonium, according to the Count, is of the form of a fig, being attached to the rocks by branches proceeding from its smaller end; its upper part being a little flattened, with a hole in the middle. Its colour, he says, resembles that of tobacco, and its parenchymatous substance, he thinks, cannot be compared to any thing better than to nutgalls, when well dried. In all these respects, a very exact agreement seems to exist between the recent and fossil substances. Still, however, the fibres running over its surface, and penetrating its substance, with the grooves which appear to have been formed by other fibres, which are now removed, distinguish it, not only from this, but, I believe, from all known alcyonia. This fossil is from Wiltshire, and appears to be formed entirely of flint.

but different.

Wholly silex.

The fossil, Plate IX, fig. 3, from Mount Randenberg, near Schafhousen, in Switzerland, possesses evident marks of its alcyonic origin. This fossil, like those of the ramose kind, figured in Plate VII, has that reticular texture, which appears to be peculiar to the spongy alcyonia. In this specimen also, as well as in those, the reticular fibres are impregnated with silica, and have their interstices filled with calcareous matter. In this, as in the fossil last described, the remains of the pedicle, the organ, by which its attachment to its appropriate spot was accomplished, are observable; as well as the superior opening, which passes into the substance of the fossil.

Reticular texture of flint filled with chalk.

Another similar.

The fossil represented Plate IX, fig. 5, and which is from the neighbourhood of Saumur, being a very perfect fossil

* Arm. 6. C. 6. p. 102. † Histoire Physique de la Mer, p. 87.

of

of the kind described by Mons. Guettard, agrees, in its general characters, as well as in its texture, with that one which has been juſt described. In this specimen, at its superior surface, there are, as Mons. Guettard observes is sometimes the case, four openings; and the pedicles, as well as its lateral processes, which appear like roots, seems to have been formed with a great degree of luxuriance.

A very perfect one of the same texture.

A very perfect fossil of this kind, and similar in its substance and texture to the alcyonia, which have been just described, but of a dark red colour, where it is not invested with its cortical part, which is of a grey colour, pervaded by a slight tinge of red, is represented Plate IX. fig. 8. The pedicle, and the opening at the superior part, are here very perfect. Slight traces of lines, passing from the pedicle to the opening, are discoverable on this specimen, and, doubtlessly point out the arrangement of fibres, by which the animal was enabled to draw in and eject the water which supplied it with food. This fossil, I have reason to believe, is English.

Fibres for drawing in and ejecting water.

VIII.

An Account of Improvements in the Culture of Vegetables, by JOHN CHRISTIAN CURWEN, *Esq., M. P. of Workington Hall, Cumberland* *.

SIR,

I AM fearful you should suppose, that I am become indolent, and that the favours so liberally bestowed on me by the Society had ceased to operate as a ſtimulus to the farther exertions of my humble endeavours to assist those objects, which by the fostering hand of the Society, have been so essentially promoted. You will excuse me for wishing to assure you that I am not idle, and to inform you that the

Objects of importance in agriculture.

* Trans. of the Society of Arts, vol, XXVI, p. 79. The gold medal of the Society was voted to Mr. Curwen for these communications.

objects which at present employ me are, I conceive, of great importance to agriculture.

The first is by experiments to ascertain the best and most productive mode of applying manure. The second is to determine, whether the distances between the stitches in drill husbandry may not be greatly enlarged, without any diminution of crop.

Best mode of applying manure.

I am strongly inclined to believe, that, where the ground is laid dry, manure can scarcely be deposited too deep; by so doing the evaporation is retarded, and consequently the manure continues for a greater length of time to furnish nourishment to the crop.

Distance of the stitches in drill husbandry.

The increase of the distances between the stitches permits the power of continuing the operations of turning up the soil to a more extended period, which not only improves the tilth, but furnishes a greater degree of moisture by exhalation, than can be yielded from ground in that state of hardness it soon acquires when undisturbed in summer. This evaporation is prodigious, though not perceptible to the eye: it is, however, fully demonstrated by a very ingenious experiment of the Bishop of Llandaff; and I am anxiously expecting to form such conclusions from trials I am engaged in respecting its effects on vegetation, as may deserve the consideration of the Society.

Feeding cattle and horses with potatoes.

My former objects of feeding cattle with potatoes, supplying milk to the poor *, &c., are pursued with increased success. The use of potatoes as a food for horses and cattle increases daily.

I am, dear sir,

Your faithful and obedient servant,

J. C. CURWEN.

DEAR SIR,

Benefits resulting from the Soc. of Arts.

IT is with great satisfaction, that I have the honour of again submitting the result of my farming operations to the consideration of the Society of Arts. Deeply impressed with a sense of the many favours conferred upon me by them, I have found myself impelled, both by gratitude

* See Journal, Vol. XVI, p. 190.

and

and inclination, to proceed with redoubled exertion, as the best return in my power.

The liberal patronage and encouragement bestowed on agriculture by the Society has powerfully contributed to awaken the country to a just estimation of its importance, as the basis of individual happiness and national prosperity; and at this moment the empire owes its preservation and security to it. Agriculture.

I submit with great deference the result of my recent operations. I am disposed to flatter myself, that they may lead to important consequences and discoveries, highly beneficial to agriculture. The experiments I have made tend to establish the double advantage of well cleaning and working the ground. First, as it frees the land from weeds; and secondly, as it conduces to the growth of the crop. It affords likewise a very strong demonstration in favour of using manure in its freshest state, by which not only the great usual expense of making dunghills will be saved, but the manure made to extend to the improvement of a third more land. Advantages of well clearing and working ground. Manure.

Most of the farm I occupy was in that state of foulness as to require, according to general practice and opinion, a succession of fallows to clean it. Being unwilling to adopt a system, which is attended with such loss, I determined to attempt to clean a part of it by green crops, and for such purpose to allow a much greater distance between the stitches, than had ever been in practice. My first experiment on this plan was made on a crop of cabbages; they were planted in a quincunx form, allowing four feet and a half between each plant, in order to allow room for the plough to work in all directions. I adopted this plan of field husbandry, as affording the greatest facility in cleaning the crop, though I believe it never was before so practised. Two thousand three hundred and fifty plants were set per acre (eight thousand is not unusual in the common method), and each plant had, by computation, an allowance of a stone of manure, or less than fourteen tons per acre; though the common quantity is generally from thirty to forty tons per acre. The manure was deposited as deep as the Foul ground cleaned by green crops. Cabbages. Manure laid deep.

the plough could penetrate, drawn by four horses, and the plant set directly above it.

Ploughed and harrowed constantly between the rows.

The plough and harrow, constructed to work betwixt the rows, were constantly employed during the summer, and the ground was as completely freed from weeds, as it could have been by a naked fallow. The very surprising weight of my crop, which in October was thirty-five tons and a half per acre, and many of the cabbages fifty-five pounds each, were matters of surprise to all who saw them, as well as to me, and I could assign no satisfactory reason for the fact. The quality of the land was very indifferent, being a poor cold clay,—the manure was very deficient of the usual quantity,—the plants when set by no means good,—in short there was nothing to justify the expectation of even a tolerable crop. I did not find any thing in the accounts from cultivators of cabbages to afford me a solution of my difficulties, or any clew to explain it. By mere accident I met with the Bishop of Llandaff's experiment ascertaining the great evaporation from the earth, as related in his admirable Treatise on Chemistry; fingular as it may appear, this very interesting experiment had remained for thirty years without any practical inferences being drawn from it applicable to agriculture. It appeared to me highly probable, that the rapid advance in growth made after the hoeing of drilled grain was attributable to the absorption of the evaporation produced from the earth, and was the cause of the growth of my cabbages. With great impatience and anxiety, as I had the honour to inform you last year, I looked forward to the ensuing season, to afford me an opportunity of continuing my experiments. I had long been a strenuous advocate for deep burying of manure, though my sentiments rested chiefly on opinion; this appeared to open a field for incontestible proofs of its advantage. My cabbages were last year planted on the same plan as the former year. Fortunately I extended the same principle to my potatoes, which I was obliged to set on wet strong ground, from want of a choice of land. My annual quantity of potato ground is from sixty to seventy acres. They were set in beds three feet long and two feet broad, leaving four feet and a half between

Great produce.

Evaporation from the earth absorbed by the plants.

Potatoes set in beds with wide intervals.

between each bed lengthways, and three feet endways. On each acre there were 1230 beds, and 6150 sets, or five to each bed, viz. one at each corner, and one in the middle. The sets of potatoes, when planted according to the usual most approved practice, in three feet stitches, and nine inches apart, amount to about twenty thousand. In the present, and indeed in all seasons when potatoes are scarce, the saving in planting is a considerable object. A great advantage also arises in being able to keep the potatoes and manure from wet. In the late uncommonly wet season I sustained little or no loss in my mode, which was not the case in many of the driest grounds. This plan unites hand hoeing with horse culture, and will be found serviceable in wet soils.

Advantages.

The lateness of planting, together with the premature frosts, prevented my forming a fair judgment as to the quantity per acre, which might be obtained by this method. My view in fixing upon this plan was, to enable me to judge of the effects of evaporation, by being able to continue my operations for a longer period. I have no doubt but that in common seasons, notwithftanding the increased distance, the whole ground would be covered.

Cabbages.

My experiments on cabbages this season commenced by planting them early in April. From the rain which fell subsequently, and continued till the beginning of May, succeeded by severe east winds, the earth became so hard and baked, that the plants had made very little progress.

Striking benefit of ploughing the intervals.

In the first week in June the ploughs were set to work: as they started, Mr. Ponsonby of Hail Hall was present, and saw the crop; it was with difficulty, that the ground was first broken, but by the end of the week it was brought into fine tilth. Notwithstanding the whole week had been dry, with a strong sun and severe east wind, yet such was the progress in growth of the cabbages, that when seen again by that gentleman on the Saturday, he could scarce be persuaded they were the same plants.

Evaporation from the ground.

During these operations I had been making constant experiments with glasses, contrived for the purpose, to ascertain the quantity of evaporation from the land, which I found to amount, on the fresh ploughed ground, to nine hundred

hundred and fifty pounds per hour on the surface of a statute acre: whilst on the ground unbroken, though the glass stood repeatedly for two hours at a time, there was not the least cloud upon it; which proved, that no moisture then arose from the earth.

The evaporation from the ploughed land was found to decrease rapidly after the first and second day, and ceased after five or six days, depending on the wind and sun. These experiments were carried on for many months. After July the evaporation decreased, which proves that though the heat of the atmosphere be equal, the air is not so dense. The evaporation, after the most abundant rains, was not advanced beyond what the earth afforded on being fresh turned up. The rapid growth of my potatoes corresponded perfectly with the previous experiments; and their growth in dry weather visibly exceeded that of other crops where the earth was not stirred. The component parts of the matter evaporated remain yet to be ascertained; the beneficial effects arising from it to vegetation cannot be doubted or denied, but whether they proceed from one or more causes, is a question of much curiosity and importance.

Evidently beneficial to vegetation.

Does not the air assist the action of the water, as in irrigation?

May not a similar process here take place, as when water is exposed to the action of the air in irrigation? Is it too much to suppose some natural operation to take place in the earth, which may decompose the oxigen contained in air from the hidrogen, during the absence of the sun, which on the sun's reappearance may be again given out in a state highly propitious to vegetation? Oxigen is found to contain carbon; and may not the growing plants imbibe it from the air, and may we not thereby account for its forming a constituent part of all vegetables?

Objects of inquiry.

The investigation of these objects presents a wide field for inquiry, and may lead to very important discoveries. From more or less oxigen contained in the earth, may not its proportions account for the fertility of one soil above another? May not the advantages supposed to be derived from loosening the soil, proceed from its being thus rendered in a fit state to imbibe the air? Fallows soon become so hard upon the surface, as to be capable neither of absorption

absorption nor evaporation. One very important result is placed before the eyes, and within the reach of every practical agriculturist to ascertain, namely, that the evaporation from dung is five times as much as from earth, and is equal on the surface of an acre to 5000 pounds per hour. By making use of dung in its freshest state, the farmer may extend his cropping to one third more land with the same quantity of manure. It is with regret that I have viewed in many parts of the kingdom the quantity of manure which is exposed on the surface, and tends to no good. I am strongly of opinion, that in all light soils, if the manure was buried in trenches as I propose, and the turnips sowed above it, more abundant crops would be procured. By cleaning with the plough, great advantage would be derived to the crop, from the evaporation yielded by the earth. Hot manure might also be used. By fermentation dung is reduced to one half its bulk, and its quality reduced in a much greater proportion. The manure now commonly taken for one acre of broad cast would, if deposited whilst hot in drills, answer for four acres, and the crop produced be much more.

Great evaporation from dung.

Dung should be used fresh and buried deep.

If the Society of Arts extend their sanction and patronage to my exertions, I shall feel bound to proceed, and to endeavour to bring the experiments to a regular system. The glasses I used for determining the quantity of evaporation were of a bell form, and placed with the open part upon the earth; a quantity of tow was first weighed, ready to wipe off the moisture collected from evaporation within the glass, which tow was then again weighed as exactly as I could after the glass had ſtood for a given time, and been wiped dry with the tow; and from knowing the contents of the glass I made my calculations. Mr. Robert Wood, watch maker, of Workington, attended to the experiments made with the glasses.

Experiments will be pursued.

I have the honour to be, with great respect,

Dear Sir,

Your obedient humble servant,

J. C. CURWEN.

DEAR

DEAR SIR,

Opinion that the evaporation greatly influences the produce.

IT is with great pleasure and satisfaction, that I learnt yesterday from Mr. Arthur Young, the Secretary of the Board of Agriculture, that he has adopted my idea of the great importance of evaporation, and that he has actually ordered Mr. Blunt, optician of Cornhill, to construct him an instrument for ascertaining the evaporation, which instrument I shall request Mr. Blunt to show to the Society. Mr. Young intends in the course of the summer to make a variety of experiments on the quantity of evaporation produced from different soils, agreeing with me, that the greater or less degree of it influences most materially the luxuriance or growth of the crop.

In all the valuable tracts which Mr. Young has given to the world, he has never adverted to this, and the first knowledge of it as a principle for promoting the growth of crops was obtained from my account of the Schoose Farm, in the report of the Workington Agricultural Society, of which he is a member.

A great saving of manure.

Being unable to account for the surprising weight of my first crop of cabbages, with only one third of the manure usually given, I was led to make the experiments I have laid before the Society; and I believe I am not only the first person in Lancashire, but even in Great Britain, who ever thought of ploughing the ground upon the principle I have executed, for promoting the growth of the crops. I flatter myself, that my experiments on the economical application of manure will lead in a high degree to facilitate a more extended cultivation, and obviate the objections, which have been started by some persons against the enclosure of waste lands, from their supposition, that manure could not be furnished for more than the land at present cultivated.

Hence more waste land might be inclosed.

I remain, dear Sir,

Your obedient servant,

J. C. CURWEN.

CERTIFICATES.

Certificates.

A certificate from Miles Ponsonby, Esq., of Hail Hall, testified, that he had seen Mr. Curwen's statement of the rapid progress made by his cabbages in the month of June 1807; that he perfectly recollects viewing them on the Monday, and again on Saturday in the same week; that the improvement in the appearance of the plants was so great, that he imagined the land had been replanted, till Mr. Curwen explained the cause, which had produced so great a change.

Certificates of the benefits accruing from Mr. Curwen's plan.

That he considers Mr. Curwen's plan of managing his potatoes and cabbages as very good garden husbandry, and the best calculated for keeping the land clean, improving the plant, and at the same time enriching the ground, of any that he had observed; and though the mode is entirely new there, he has no doubt but it will be found beneficial, and that it will in a few years be much attended to.

A Certificate from Mr. D. Campbell, Secretary to the Kendal Agricultural Society, stated, that he had attended to the cultivation of potatoes in most parts of Lancashire, and could speak with the greatest precision respecting it in that part of the country which is north of Lancaster.

That whether they were planted in the lazybed way, by the dibble, or with the plough, they were always set in *rows* from *one end of a field*, or *piece of ground*, to *the other end or side*, with narrower or wider intervals, as the cultivator might deem best suited to the kind of potato he was raising. That he never before saw or heard of their being cultivated in beds, in the manner practised and described by Mr. Curwen, and that being more particularly desirous to ascertain whether any such method was pursued in the great potato district which lies south-west from Lancaster, including Pilling, the Felde, Rufford, and the neighbourhood of Preston, he applied to George Clayton, Esq., of Lostock Hall, and Robert Hesketh, Esq., of Warrington Hall, gentlemen upon whose accuracy the utmost dependance may be placed, and who informed him, that neither from their own knowledge, nor from inquiries they have made, can they learn that

that the method of cultivating potatoes alluded to has been seen or heard of in a tract of country, where more are raised for the market than in any other of the same extent perhaps in the kingdom.

Advantage of Mr. Curwen's mode of planting cabbages.

Mr. Campbell further stated, that Mr. Curwen's cabbages were planted at a much greater distance than any he had ever before seen, and their size far exceeded, as a general crop, any that had fallen under his observation; that the ground was perfectly clear from weeds, and from having been frequently turned over by the plough in the intervals, the mould appeared to be in fine order for a subsequent crop, and he conceived that in the two essential points of freedom from weeds, and of the land being in a fine tilth, no garden could exceed it.

Farther certificates.

Other certificates respecting the novelty of the method of planting potatoes, as practised by Mr. Curwen, were received from the following gentlemen:

William Knott, Summerhill.
Mr. Sunderland, Ulverston.
J. Penny Marshall, Bolton Oak.

Further certificates, stating the method to be new as practised by Mr. Curwen, for planting both potatoes and cabbages, were received from the following gentlemen:

Walter Gardner, Crooks.
William Harrison, Ulverston.
A. Benson, Reading.
Henry Richmond Gale, Bardsee Hall.
Jos. Penny, Budgefield.
Edward Barrow, Allithwaite Lodge.
Charles Gibson, President of the Lancaster Agricultural Society.
Rev. J. Barns, Pennybridge.
Rev. E. Ellerton, Colton.
Jos. Yorker, Ulverston.
Michael Knott, Thurstonville.
Rev. Joseph Brooks, Ulverston.
Thomas Machell, Aynsome.

Also

Also from the following farmers, resident in the neighbourhood of Lancaster:

THOMAS TART.
WILLIAM ARMSTEAD.
WILLIAM STALLER.
ANTHONY EIDSFORTH.
CHRISTOPHER ATKINSON.
ROBERT EDMONDSON.

DEAR SIR,

Mr. Curwen having informed me, that a question would probably arise in the Society of Arts &c. relative to the degree of exhalation of water from the earth, and it appearing to me to be intimately connected with various matters in agriculture, I think you will not be displeased at my mentioning a few circumstances, to prove, that the object much deserves attention. I conceive that it bears upon the point of showing the great depth, to which dung may be ploughed with safety; for when we find, as I have done, that from two to three thousand gallons of moisture are exhaled in a day from an acre of land, and that the quantity varies greatly according to the state of tillage, it should appear, that such a vertical stream of vapour must remove all apprehensions of *burying* dung. I also think it goes to the point of hoeing and horse-hoeing such plants as demand much moisture. I have found, that the dung in a farm-yard, laid three feet deep and hard trodden by cattle all the winter, has exhaled in the proportion of above four thousand gallons per acre in ten hours; hence a practical conclusion may be surely drawn. I could much extend these observations, but they are sufficient to convince so enlightened a mind as yours of the propriety of a very extensive pursuit of this inquiry.

Subject of evaporation of much importance.

I have the honour to be,
With much regard, dear Sir,
Your faithful and very humble servant,
ARTHUR YOUNG.

IX.

IX.

*Electrical Experiments on Glass considered as a Leyden Phial, and on coated Panes; by Mr. *** *.*

Experiments militate against the doctrine of plus and minus electricity.

CHANCE having thrown in my way two papers written in Dutch by Mr. Lugt, I was surprised on reading them to find, that this gentleman could admit the theory of plus and minus electricity, while almost all his experiments concur in proving, that there is an actual passage through the pores of the glass, when it has a communication on one side with the prime conductor of an electrical machine in action, and on the other with conducting bodies communicating with the ground: and that to obtain this passage it is not necessary for the glass to be coated on both sides, as it is sufficient for that in contact with the machine to be so, and to touch at a single point some substance that is but an imperfect conductor, as the wood of a table, or the like, which has sufficient force to communicate the attraction of the Earth through its pores. Thus I have always suspected the charge of the cascade is effected, in the 5th experiment of my first letter to Mr. van Mons: but as the phial seems to retain in its pores a portion of the electric fluid, and collect on the surface communicating with the ground a large quantity of fluid sensible both to the touch and sight, when we charge highly a phial not coated on that side; I have thought the force of attraction of glass for this fluid was so powerful, that Abbe Nollet had reason to suspect it attracted electricity from the Earth, which however did not happen in the experiments of Mr. Lugt, as for instance the following, which is the second of his first essay.

Glass has a powerful affinity for the electric fluid.

Insulated phial charged by an insulated machine.

He procured an apparatus completely insulated by means of four glass feet. Thus he could at pleasure leave the whole insulated; or form a communication between the ground and the conductor, or the ground and the rubbers, which were united together by a semicircle of metal placed about a foot from the insulated plate. Rods were contrived

* Journal de Physique, vol. LXIV, p. 371.

to

to be fixed occasionally to the conductor or the rubbers. In this experiment he fastened one of these rods to the rubbers, and made it communicate with the outer or inner coating, it did not signify which, of a phial placed on an insulating stand; the other coating of the phial communicating with a similar rod fixed to the conductor. The communication was made by means of a wire in contact with each coating, and terminating at the other end in a knob, which might be brought near or removed from the other rods at will. This phial, thus completely insulated, was charged by an equally insulated machine. Hence the author infers, that the ground does not contribute to the charge of the phial; and that, when the apparatus is not insulated, the wood of the table, and that which supports the stand, are the invisible conductors of the fluid from the surface that parts with it towards the point where the fluid is excited on the plate: that in his insulated experiment the use of the rods supplies the place of the ground, and conducts the fluid: &c.

Glass not impenetrable to the electric fluid.

I cannot admit the theory of taking the fluid from the surface of an impenetrable substance, as Dr. Franklin asserts glass to be; because it is a fundamental law of chemistry and physics, that no movement can take place without a previous impulse, and consequently without immediate action on the substance to be deprived of the fluid. Besides, what substance is there, that the igneous matter cannot penetrate? and no one will deny, that the igneous matter forms a part of the electric fluid. Accordingly I deduce an opposite inference from this experiment.

Affinity of glass for the electric fluid shown by

Mr. Lugt then recites several very ingenious experiments, among others the following on the electrophorus, by which he would go on to prove this singular deduction; but which in reality prove nothing, except that the attraction of the igneous fluid, developed at the disk, is strong enough to supply the place of the attraction of the ground: in fact, that in uninsulated and insulated experiments glass has such an elective attraction for the fluid, as to retain the same quantity in both situations of the phial. It is still to be accounted for, like all chemical and physical phenomena, by the theory of elective attraction.

He

an experiment with the electrophorus.

He takes an electrophorus, places it on an insulating stand, and insulates himself before he rubs it. In this state of complete separation from the ground he excites it by friction, touches the two coatings, and obtains sparks as strong as if both he and the electrophorus had a communication with the ground. Hence he concludes still, that the double contact, necessary as he says, establishes a complete circulation, as in his experiment with the phial.

The experiment made in another way.

There is a more simple mode of making this experiment with a small curved exciter with a glass handle. I take an electrophorus completely insulated; I rub it in a state of insulation like the Dutch philosopher; I quit the insulating stool and take the exciter, the two coatings of which I touch at once with its knobs; and I not only obtain a spark, but taking the exciter, leaving one of its knobs on the external coating, and raising the other four or six inches so as to lift the cap to it in the air, a real discharge takes place. On laying down the cap without a fresh contact, scarcely does it give a very feeble spark. The beautiful experiments of Mr. Libes, in which he obtains electric fluid by the mere contact of different metals, evidently prove to me, that here, where the action is triple, or between two metals and rubbed resin, there is a real generation of igneous matter, if I may so express myself, which is renewed at every double contact. The following experiment is calculated to support my conclusion.

Electricity from the contact of different metals.

Sparks from the mouldings of a chest of drawers when one taken from an electrophorus upon it.

I had seen in the Electrical Phenomena of Mr. Sigaud de la Fond, that some gentleman observed the gilt mouldings of a chest of drawers to emit sparks every time he drew one from the cap of an electrophorus accidentally placed on it *. This fact led me to make the experiment with an insulated electrophorus, by the side of which I placed a copper ball having a rod that communicated with the ground. This ball was about a line from the outer coating; and I stood on an insulating stool when I took a spark. In this state, to prove that it is no circulation that occasions the discharge, but an attraction of the ground, which becomes divellent at the moment when the fluid retained in

* Phénomènes Electriques, p. 678, § 174.

the

the metal of the cap acts no longer in competition with the glass to fix it in the metal of the inferior coating, I raised the cap three or four inches, and held it thus a few seconds without seeing the least spark pass between the inferior coating and the knob of the exciter; but the moment I drew electricity from the cap, a strong spark was emitted toward the ground. This fact gave me the more pleasure, as it still more confirmed the theory of elective attraction, on which all my deductions are founded. I know not whether this experiment be new, but I do not find it in Libes, Haüy, or the French translation of Fischer, which has lately appeared with notes by Biot; and it appears to me to merit attention, as it throws light on the theory of thunderstorms. Here the column of air interposed between the cap and the glass prolongs the retaining power of the glass to six, eight, or even fifteen or sixteen inches in dry weather: there I figure to myself a large plate of air between those clouds that traverse the atmosphere in opposite directions, the electric fluid of which remains insulated till the moment when the elective attraction surpasses the retaining action of the stratum of air, &c. This experiment also shows the reason why the new doubler of electricity, invented a few years ago in England, charges its plates on approaching and separating them repeatedly, and acquires through the stratum of air that separates them so intense a change, that the plates discharge themselves spontaneously *.

Thunderstorms.

Doubles.

The glass electrophorus, mentioned by Mr. Lugt as well as Sigaud de la Fond, but the effects of which, as it appears to me, have not been compared with those of the Leyden phial, has lately engaged my attention. The following are the experiments I have been led to make, and in my mind they render still more probable the complete saturation of the Leyden phial by the retaining affinity of the substance of the glass itself.

Glass electrophorus.

I take a square of German sheet glass [*verre blanc de Bohême*] twenty or two and twenty inches wide, and place it on an insulating stand seven or eight inches in dia-

Experiment.

* See Joumal, vol. IX, p. 19. It is for September, 1804, not 1805, as misquoted by the writer in the Journ. de Physique.

meter, gilt or silvered all over, with its edges well rounded off, and supported by a glass foot at such a height, that the balls of the two curved tubes may rest on a little metallic circle of three or four inches diameter cemented to the centre of the upper side of the glass. Below I place a knobbed exciter against the edge of the gilt top of the insulating stand, leaving about a line distance between them, as in the preceding experiment. In this state I begin to charge. At the first turns of the plate it frequently happens, that we see round the little upper coating some flashes of electric light; but if the glass be thin, they will soon disappear, and though you continue to turn the plate a thousand and a thousand times, the square will be charged to the whole capacity of the coated glass, but will afterward yield a continual passage to the fluid. By this experiment in the dark I have been convinced of the reality of the passage of the fluid through the pores of glass as through a filter of capillary tubes. This experiment was repeated several times in the presence of the friend, who suggested to me the idea of the oxidation of the metallic coatings, comparing them with those, which probably take place in the great in marble quarries. He is inclined to consider this as an *experimentum crucis* with respect to this passage of the fluid. It is thus he is equally convinced, that the electric fluid oxides the most tenacious metals partially in its passage, before it destroys them at the instant of the developement of the gasses, which takes place in my metallic cylinders. He is an excellent pneumatic chemist, and frequently repeats to me, that caloric penetrates all bodies, that all consequently have pores, and that the penetration of the electric matter through those of glass is in no way inconsistent with the true principles; but that the pretended removal of it from one side of the glass, which receives a superabundance of it on the other, is contrary to the axiom of his master, Lavoisier: there is no motion, no sensation, unless the impulse acts through the thickness; and hence, if we grant this expulsive action, we must admit a capacity of penetration in the fluid.

Passage of the fluid through the pores of the glass.

Electricity destroys metals by oxiding them.

In his presence I repeated the experiment with common glass. This yields a passage to the fluid with less ease, but on

Common glass less easily perváded but

on the contrary saturates itself infinitely more quickly: in a little time it discharges on itself, notwithstanding the little extent of the coatings. We ascribe the anomaly of these two different kinds of glass to different fluxes. The German glass contains more metallic oxide, the common more saline matter. If this inference be just, the English flint glass should be like a sponge to the fluid; and if it were possible to find large squares coloured with metals, these perhaps would furnish us with other facts.

sooner saturated with the electric fluid.

Flint glass.

It must be observed, that, notwithstanding the German glass admits this passage, a large mass will not pass, unless it be attracted in the manner related in a former letter. This is why we see a reflux toward the machine. The following experiment will in some degree account for this.

German glass.

I charge a glass electrophorus, placed on an insulating stand, the lower coating of which is as extensive within an inch as the glass, and stop the machine the moment the sparks announce an approaching spontaneous discharge: if in this state I cut off the communication with the ground, and take the cap from the upper surface, the whole charge will remain adhering to the glass; and on touching it a prickling sensation will be felt, and something like an igneous vapour. On extinguishing the light it is visible, particularly if you approach the edge; but the fluid becomes absolutely luminous, if you blow lightly on the surface: then a wave of fire traverses the glass, to join the fluid accumulated on the other side between the glass and the metallic coating. What is particularly remarkable, two colours may be distinguished in the fluid, the lower being whiter and more vivid. This phenomenon takes place if the communication be suffered to remain: the wave of fire, which flows from the part blown upon toward the lower surface is stronger, but it does not continue so long. This experiment gives rise to the question, whether all the ingredients pass through the substance of the glass, or whether the difference of action is to be ascribed to the state of the glass alone. I believe it is this modification, which the electric matter itself appears to undergo, that constitutes the opposite states, which every natural philosopher endeavours to explain according to the mode in which he views them;

Experiment.

The fluid may be blown toward in a luminous wave

of two colours.

The electric fluid or compound.

them; Franklin by plus and minus; du Fay by two fluids neutralized in bodies, the particles of which repel and attract each other; &c.

Is not the fluid retained by the attraction between the two surfaces of the glass?

Does not this experiment demonstrate, that the attractions, which act here between the surfaces of the glass reciprocally, retain the fluid on the upper side notwithstanding we take off the cap? while, if the opposite surface be not insulated, the cap takes it off at a distance of three or four lines above it, if we touch the cap with a metallic body communicating with the ground without establishing a complete circuit; because then the ground wholly absorbs that which is accumulated on the opposite surface. To verify this fact, I have repeated the transvasation of water, in the three following manners.

Water poured from a charged into an uninsulated bottle,

I charge a bottle filled with water, and pour the water into another bottle standing on a plate of lead, that has a communication with the ground. Whether I be insulated or not, when I do this, the two bottles divide the charge between them. But to retain the charge in that which has lost its water, I must place myself on an insulating stool when I pour into it fresh water, unless it be from a glass vessel; otherwise, as the electric fluid may escape both by my body, and by the metal on which its outer surface rests, and which can conduct the opposite electricity into the ground, the bottle will discharge itself entirely on one side by my body, and on the other toward the ground; in the same manner as a charged bottle touched by the hand, while there is a communication between the ground and its opposite side.

and into an insulated bottle.

On the contrary, if I charge a bottle highly, and pour its water into an insulated bottle, the water will convey away nothing, and the whole charge remains in the bottle; because there is no attraction of any substance to act on the electric fluid, the glass, which I suppose to be saturated during its fusion, having no longer any affinity to attract it. It is like a full sponge, which takes up no more water, unless it can part with some of what it contains to another body. It is not in the coatings then, that the fluid is retained, but in the glass itself, and on its two sides. If, as I have remarked above, I make the tranvasation into a bot-

tle

tle communicating with the ground by its external coating, while I stand on an insulating stool, it neither loses nor acquires more of the electric fluid. Must we not hence conclude, that the outside, when once charged, neither attracts any thing more from the ground, nor gives off any thing to it?

The following experiment with the electrophorus throws still more light on all these facts.

I charge an electrophorus of glass and resin; I touch it on both sides; I raise the cap, and place it again on the electrophorus; the moment I touch with my hand either the external coating or the cap, I perceive a spark almost as strong as that which issues from the cap taken off. But if, before I replace the cap, I touch the inferior coating, I take from it its superfluous electricity; and when I touch it afterward the spark is almost nothing: a sign, as it seems to me, that the fingers in touching the two surfaces only establish a communication between the two coatings, which serves as a divellent intermedium, if I may use the expression, to develope the fluid that is disengaged.

Experiment with the electrophorus.

I offer these views to the natural philosopher, not to create a new theory, but as an inquiry whether the igneous phenomena of magnetism, galvanism, electricity, and detonations, be not subordinate to the general law of affinities. The fine experiments with which Libes and Ermann have enriched the fields of science concur in support of the hypothesis, that there is but one igneous matter, which forms light, the magnetic, galvanic, and electric fluids, &c., and is modified in them by different ingredients. In a letter which I wrote to Mr. Delamétherie about six months ago I called these *semigravitating*, because I see them always take a centrifugal force, and accompany this matter when it is disengaged from combustible bodies: one of these fluids takes it, like that of ether, at a certain degree of heat; another only at the strongest heat of a burning glass, unknown before Homberg, and even in his time, which is necessary to volatize gold; and so on. The experiments of Mr. Ermann demonstrate, that the flame of alcohol contains different ingredients from that of sulphur, or that of phosphorus.

Does not the matter of fire, combined with different ingredients by chemical affinity, produce the phenomena of magnetism, electricity, &c.?

phosphorus*. Examine the gasses, which the same acids evolve from different metals, or the different colours of artificial fireworks; do not all these modifications demonstrate, that the caloric of the air, added to the ingredients latent in combustibles, carries off various particles, the number of which will ever remain unknown to us? Of the nature of carbon, nitrogen, hidrogen, oxigen, abundant as they are, we are still ignorant. Are they simples? or are they compounds? How many varieties do these four bases afford merely by the proportions in which they are combined? Why does the new inflammable mixture that alarmed Proust, and prevented him from pursuing his experiments, appear still more terrible than fulminating silver? Before my experiments, if I had spoken of the combined action of water, lead, and the electric fluid on the most tenacious metals, as solders and iron, should I have ventured to say, that the igneous expansion in them might at length become sufficiently powerful to burst a cylinder of the best iron of ten lines in diameter, and two lines aperture, consequently four lines thick; as well as a large cartridge of an alloy of nine parts copper and one tin similar to the former, which so long resisted a force of about forty feet, and was burst by one of a hundred and forty in ten explosions? That of iron exhibited undulations at the ninth explosion, but was not actually cracked till the fortieth. I could wish, that some one would try two cylinders of similar materials, to find the proportion of the resistance, which is not in the ratio of the square of the thickness, as I had imagined. The progress of the resistance is greater on doubling the thickness of the iron; for a cylinder of iron of half the thickness was cracked at the fourth explosion, and at the seventh the cracks were wider than at the fortieth in the thicker cylinder. I cannot but be persuaded, that the igneous action tends to decompose the metals subjected to it.

Action of water, lead, and the electric fluid, will burst any metals.

* Journal de Physique, February 1807: or our Journal, vol. XVII, p. 246.

X.

IX.

On the Identity of the Base of Charcoal with Hidrogen, or its Base. In a Letter from Dr. JOHN NEW.

To Mr. NICHOLSON.

Stapleton, near Bristol, April 24th, 1809.

SIR,

IN the 18th volume of your valuable Journal, p. 43, is inserted a paper, entitled " Report on a Memoir of Mr. " Berthollet, jun. entitled, Inquiries concerning the reci- " procal Action of Sulphur and Charcoal; by Messrs. " Fourcroy, Deyeux, and Vauquelin." Reciprocal action of sulphur and charcoal.

The general conclusions from the experiments are,

" 1st. That charcoal contains hidrogen, which the most " intense heat we can produce will not completely expel." Charcoal contains hidrogen.

" 2d. That sulphur at a red heat acts upon hidrogen, " and forms compounds in very different proportions, on " which their properties depend." Sulphur forms a compound with hidrogen,

" 3rd. That charcoal deprived of hidrogen, or at least " nearly so, forms with sulphur a solid compound, into " which the sulphur enters in a small proportion." with charcoal depri ed of hidrogen,

" 4th. That at a high temperature sulphur, carbon, and " hidrogen unite into a compound, which assumes the state " of gas." or with both;

" 5th. And lastly, That sulphur contains hidrogen." and contains hidrogen.

The perusal of this important paper furnishes me with an opportunity of communicating an opinion, which I have, for some years, entertained: *That charcoal and hidrogen are modifications of one and the same substance*, or *that hidrogen is the base of charcoal.* Hidrogen the base of charcoal, or a modification of its base.

My opinion was formed from tho result of various experiments and obervations, made at a time when experimental chemistry was a favourite amusement; but which very different pursuits have obliged me reluctantly to relinquish. This opinion founded on experiment,

Should this opinion be confirmed by accurate experiments, (and it appears to me to have been nearly proved by Berthollet in the Memoir above quoted, at least by analysis)

lysis) what an important and extensive field will be opened to the scientific world!

The carbon of plants from water.

The pabulum of plants, and the origin of that immense quantity of carbonaceous matter annually produced in the vegetable kingdom, will be easily and satisfactorily accounted for, as originating from *water* alone.

Different appearance of hidrogen and charcoal no argument.

Although the two substances hidrogen and charcoal differ so much in appearance, yet, it may be a question whether the *diamond* and *charcoal,* or *steam,* in its greatest degree of rarity, and *ice* or *snow,* do not differ quite as much.

This intended only as a hint for inquiry.

I do not mean by this communication to lay claim to any priority of discovery, but only to furnish a hint to others, which, if improved by those who have leisure and ability to pursue the inquiry, might lead to the discovery.

I took no notes of the experiments to which I have alluded, and certainly cannot, at this distant period, narrate them from memory; and, if I could, it is by no means improbable, that they might be explained in a different manner.

I am, Sir, your obedient servant,

JOHN NEW, M.D.

X.

Extract of a Letter from a Gentleman in Jersey to his Friend in Glamorganshire, on the Use of Vraic as a Manure. Communicated by J. FRANKLEN, Esq*.

Seaweed good manure for light soils; its ashes on strong.

VRAIC, or its ashes, we esteem here good for all manner of soil, whether deep and heavy, shallow or light; for we use it on all our lands. I think ashes agree best in the strong soil, as they lighten it, and open its pores; and the vraic in the light or shallow soil, for it keeps it moist in the summer: yet our people use both together on all lands. The ground receives no benefit from the vraic but for the

* Bath Society's Papers, Vol. X, p. 258.

year

year in which it is laid on; but does from the ashes for several years.

Our time of gathering it in summer is always the first or second spring-tide after Midsummer: the Court fixes the day to begin to cut it. There are but six or seven days allowed to do it. It is done with a small hook, partly cut and partly torn from the rocks. It is brought ashore just above high-water mark, and there spread and dried in the same manner as hay. Three or four days of fine weather are enough, (for it must not be too dry.) It is put in large cocks, and carried home at leisure, and housed. If there be no convenient place they make a rick, and a certain quantity is brought within at a time. A small bundle of brambles, or a little faggot, is put in the chimney, and twice or thrice as much vraic as a man can take in his arms placed over it. It makes a good fire, and as it burns must be supplied with fresh vraic. The ashes must be drawn aside in a corner of the chimney every now and then, for it must not be burned too much, otherwise it would lose the best part of its virtue. The ashes are carried away every morning to a place under cover. Before I leave this article, I must observe to you, that it may be gathered with you, as there is no restraint, any time in the summer.

Collecting and curing it.

Burning for ashes.

The winter vraic is begun to be gathered about the middle of February, and continues till about the latter end of March. That with large broad leaves, which usually grows in deep water, is the best to be used green. It is carried as soon as possible on the land for which it is intended, and spread on it, if rainy weather. If very dry weather, it is left on the ground in little heaps till moist weather.

Spring gathering.

This is the method by which we gather our vraic here. Now I will describe how we use it. After our land has lain fallow three or four months, about December or January we give a light ploughing, just to turn the turf. Some spread their ashes before it is turned; others after. I believe it is no great matter which. We allow forty-eight bushels to a vergee, (two vergees and a quarter make an English acre) the green vraic is brought, as before-mentioned, and spread in such a manner as that the leaves almost

Method of using it.

almost touch one another. We generally allow two cart-loads, or sixteen horse-loads, to a vergee.

Crops. In the latter end of March, or beginning of April, this ground is ploughed deep, and sown generally with barley. Some sow a sort of wheat which we call *fremé*, which must be sown the beginning of March; others sow the common red wheat in the beginning of December, allowing the same quantity of ashes; but instead of vraic they put dung. This is the way of our ploughing the first year. The second year the soil is manured and ploughed as the first, but always sown with barley, at the season before-mentioned. The third year there is no manure used, nor the following years. All the ground is either dug with a spade, or turned with two ploughs, one following the other in the same furrow, that the ground may be turned deep. In January and February beans are planted in ridges, and parsnips sown all over the ground; the weeding and digging of which is very expensive; but nothing that I know answers better than parsnips to fatten hogs or black cattle.

This ground that has been dug deep, stirred in the weeding, and again dug to get the parsnips, is finely prepared to sow wheat the fourth year, which is done in December and January. I generally sow clover seed in it in the beginning of April, which I think better than taking oats the fifth year; for it impoverishes the soil, and its produce is not answerable. However, most people sow oats after their wheat and clover seed.

Produce. Now as to the produce. This cannot be exactly ascertained, as it depends on the nature of the soil, goodness of the season, &c. So I will fix it as near as I can at a medium. Of barley, we have sixteen bushels per vergee, each bushel fourteen gallons; of beans, about eight bushels (same measure) per vergee; and five cart-loads of parsnips. The produce of wheat is about fourteen bushels, of ten gallons each, per vergee. We have about the same number of bushels of oats, at fourteen gallons each.

XI.

Account of an extensive Orchard planted at Bradwell in Essex, by Mr. SAMUEL CURTIS, *of Walworth**.

SIR,

I Take the liberty of sending you an account of an undertaking, for which I hope I shall be entitled to some notice from the Society of Arts, &c. I do not know whether they have offered premiums or medals for planting fruit trees, nor do I suppose it is always requisite, as I understand the Society confer their favours without such offers for matters they think deserving of them.

Farm of fifty acres converted into an orchard.

Two years ago I took a small farm in Essex, (a county where fruit is scarce,) consisting of near fifty acres. As the soil appeared proper, and the aspect favourable, I converted the whole into an orchard, by planting one hundred trees on each acre, in the following manner, viz. The fruit trees are placed in rows one rod asunder; between the trees in each row is a space of two rods; the plants are cherries, and apples or pears alternately, so that one half of the plantation consists of cherry trees. In about twenty or thirty years the apple and pear trees will require the whole of the ground; the cherry trees are then to be cut out, leaving the apple and pear trees uniformly two rods asunder each way, and in straight lines.

Farming crops produced on the ground as before.

The orchard is now completed with the best kinds known or produced in the nurseries, in the whole nearly five thousand standard trees. They are well staked, and have been properly pruned twice a year. Farming crops have been since produced on the same ground as good as formerly, the plough being allowed to go within two feet of the trees each way, so that for many years to come the land will pay the expenses, and yield a profit exclusive of the fruit. I have in one part planted medlars, quinces, plums, walnuts, and other trees, to make the fruit collection as complete as possible, and I have spared no expense which could tend to improve the whole.

* Trans. of the Society of Arts, vol. XXVI, p. 123. The silver medal of the Society was voted to Mr. Curtis for this communication.

I shall

Destruction of insects.

I shall make it an object to destroy the coccus, an insect which is at present damaging all our orchards. I know the application of spirits of turpentine will do it, without injuring the trees; it is by far the most easy and expeditious method for that purpose.

I am, Sir, your obedient Servant,

SAMUEL CURTIS.

Testimonies.

Certificates from M. P. Carter, D. D. Rector of Bradwell, and Mr. Thomas Fairhead, Churchwarden, confirmed, that Mr. S. Curtis had planted about four thousand standard fruit trees on about forty-eight acres of land, and that the same were, on the 7th of April, 1808, in a thriving condition.

SIR,

THE certificate I sent you relative to my orchard stated the number of trees to be about four thousand, but the real number is 4620 trees. I am sorry to have occasion to notice to you a disease in pear trees, almost as destructive, although not so frequent as that I mentioned to be produced from the insect on apple trees. This upon pear trees appears as a dry rotten scab, which keeps increasing until it penetrates even the hard wood, and as it proceeds, surrounds the limb entirely. The following spring the limb dies from the diseased part upwards. I have not found any insect to be concerned in this disease, which frequently takes place upon the trees of most luxurious growth. Its commencement seems to be from the thick rind of the tree becoming spongy; it then begins to crack and look scabby, the inner bark becomes dark coloured, and the disease proceeds until the destruction of the limb takes place. Some particular sorts of pear trees are with me much more liable to this disease than others: Windsor, autumn, bergamots, Catharine pears, &c. I suspect the disease to arise in a great measure from the soil.

Disease in pear trees.

My new orchard is situate at Glazen Wood, near Coggeshall, in Essex. I think myself highly honoured by the inquiries of her Serene Highness the Margravine of Anspach concerning it. As her Highness has attended to the pruning of fruit trees both in England and on the Continent, doubtless she is aware of the existing diseases in apple and pear

Remedy for this, and that of apple trees wanting.

pear trees,—any easy remedy for them would be of immense consequence; and if her Highness can furnish any discovery relative thereto, it would confer a great service on the public, and be esteemed by me a very particular obligation and honour.

I remain, Sir, your obedient servant,

SAMUEL CURTIS.

XII.

On the Management of Marsh Lands, Irrigation, &c. in a Letter to a Friend. By Mr. Thomas Davis*.

SIR,

WITH respect to the management of Marsh Lands *after draining*, the great desideratum is to make them perfectly dry—to get rid of the coarse aquatic grasses, and to replace them with the finest and best grasses; and as the latter root is much shallower than the former, they cannot be made to thrive, unless the land is firm and close round their roots. There are but few instances where land of this description does not contain plenty of the best grasses, but in such a weak and starved state, that you can scarcely see them until the land is drained, and made so firm in its surface as to discourage all the coarser, and encourage the finer grasses, by bringing vegetation near the surface, and affording a proper nidus for the small shallow root of the latter. Management of marsh lands after draining.

But between the decay of the coarser grasses and the establishment of a better kind, there will be an *interregnum*, in which the land will be worked very little; *in some instances less than before it was drained at all.* The enclosures and drainage of the marsh lands (called moors) in Somersetshire, and the fens in Lincolnshire, have shown this clearly, and the same cause must produce the same effect every where. At first generally appears worse.

For the first three or four years after the drainage, the land has generally grown gradually worse; for two more, it has been stationary; and then, if well managed, and particularly by the help of a dry summer, it has improved rapidly, and will never, unless shamefully neglected, revert to the former state. Afterward improves.

* Bath Society's Papers, vol. X, p. 324.

But

This improvement may be accelerated.

But this *interregnum* may be much shortened, by reflecting on its cause, and acting accordingly. If the coarse grasses are to be destroyed, they must not be suffered to seed. If the shallow-rooting fine grasses are to be encouraged, the earth must be trodden into contact with their roots; of course mowing should be avoided, and feeding *in dry weather, as hard as possible, encouraged*; and the stock should be that of the *cow kind*. Horses eat very unfairly, and are continually running about and poaching the ground; and sheep will pick out all the fine grasses, and leave the coarse. But the surface water must mostly be drained off; and feeding in wet weather, particularly in the winter, avoided as much as possible.

The under water must be complete.

I am supposing all this while, that the land has been completely drained of its *under-water*, or else it is useless to attempt any thing towards its improvement. Manure may as well be thrown into the water, as put upon land, which (though not always under water) *is full of water* every winter. Besides, the *under-water* of marsh land, particularly under the hills which contain veins of blue lyas stone, as in Lincolnshire and Somerset, is frequently so impregnated with sulphur, as to be injurious to vegetation; and the land never improves much, till this water is completely drained and kept out of it.

Suitable manure requisite.

When land of this description is recovered, and well stocked with good grasses as above described; these grasses should be encouraged by such manures as suit the soil, such as wood-ashes, peat-ashes, soot, and other top-dressings in the spring, till the grasses are completely established; and then lime, chalk, marl, clay, sand, or whatever suits the land best, may be used in large quantities as *alterative manures*, but not until there is *a good coat of grass on the land*. In the choice of these manures, local manures will be useful; theory, on the soundest principles, is sometimes fallacious. But the golden rule of agriculture—to use such manures as will make heavy land lighter, and light land heavier; cold land hotter, and hot land colder—must never be lost sight of. He that knows and follows this rule, and he only, is a farmer.

Principles of manuring.

If any of your land be capable of irrigation, and you have *water*

water enough to do it properly. (the great errour has been in attempting too much land with a given quantity of water) no improvement can be so great. But the land must not only be first drained of its under-water, but must be by nature, or made by art, capable of draining itself, and that speedily, from the water to be brought on by irrigation, or the attempt should not be made; and marsh land is seldom in this shape, unless *a river runs through it*, and there is of course a natural fall in the land: where you have this advantage, embrace it by all means; if you have not, be shy of attemping any thing on a large scale, until you have consulted some one who perfectly understands the subject. With all the improvements to be derived from irrigation, (and it certainly is the greatest improvement in agriculture) local prejudices, in countries where it is but little known, are strong against it. Every thing may look favourably, and yet the water may not agree with the land, or the land with the water; and the owner may be put to a great expence, and not only be disappointed, but what is to the full as vexatious, be laughed at by all his neighbours. Begin therefore with a little, and do that little well. You must not pretend to undertake irrigation by any written instructions, which I or any one else can give you. You must get a man who understands the subject practically, and who will undertake it at a fixed price per acre. But even then I would do but little at first, then wait a year, and see the effects, before I would go farther. And by the by, it is absolutely necessary, that your own workmen should see the effects, and understand the subject, and be fond of it; for every farmer, let him profess what he will, is governed by his own workmen; and whatever he may attempt to do will never fully succeed, unless he can get them to like it as well as himself.

Irigations.

Necessary to convince the minds of workmen.

I am, &c.

Horsingham, Oct. 1805. THOMAS DAVIS.

METEOROLOGICAL JOURNAL

For *APRIL*, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

Day of MAR.	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
26	42	44	48	38	29·17	Fair	Fair
27	44	46	52	40	29·30	Ditto	Ditto
28	45	42	50	39	29·45	Rain	Rain
29	42	40	45	35	29·72	Fair	Fair
30	38	42	44	38	29·83	Ditto	Ditto
31	42	40	49	36	29·79	Ditto	Cloudy *
APRIL							
1	40	39	45	31	29·73	Ditto	Fair
2	38	36	43	29	29·80	Hail †	Ditto
3	34	37	43	28	29·90	Ditto	Ditto
4	35	28	40	32	30·05	Ditto	Ditto
5	34	36	42	30	30·26	Ditto	Ditto
6	36	36	44	30	30·27	Rain	Rain
7	38	39	46	28	30·16	Cloudy	Cloudy
8	40	42	50	37	30·33	Fair	Ditto ‡
9	42	46	52	44	30·14	Ditto	Rain
10	46	50	56	45	29·91	Rain	Cloudy
11	48	42	53	32	29·56	Ditto	Ditto
12	40	43	48	40	29·80	Fair	Rain §
13	42	44	52	38	29·32	Rain	Cloudy ‖
14	42	45	46	40	29·09	Ditto ¶	Ditto
15	43	46	50	40	29·47	Fair	Ditto
16	46	47	53	41	29·08	Ditto	Rain
17	41	40	43	34	29·13	Rain	Ditto
18	36	37	42	32	29·57	Hail	Fair
19	34	39	43	35	29·77	Fair	Ditto
20	40	38	42	35	29·73	Snow	Rain **
21	38	40	42	39	29·58	Ditto	Cloudy
22	44	46	47	45	29·67	Rain	Fair
23	43	41	46	40	30·01	Ditto	Cloudy
24	41	43	48	38	29·28	Fair	Ditto

* With very cold wind.

† From 9 A. M. to 1 P. M. the thermometer rose 4°, and during the heavy storm of hail fell to 38, and afterwards rose to 43.

‡ Venus and Mars visible at times.

§ Very high wind at 11 P. M.

‖ Lightning at 9 P. M.

¶ Thunder at half past 6 A. M.; again, with hail and lightning, at 1 P. M.

** Heavy snow at 7 P. M.; again, during the night of the 21st.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1809.

ARTICLE I.

Observations on the Natural History of the Divers. In a Letter from PATRICK NEILL, *Esq., Secretary to the Wernerian Natural History Society.*

To Mr. NICHOLSON.

DEAR SIR,

HAVING paid some attention to the natural history of the Divers, I have subjoined some remarks in answer to your correspondent's inquiries concerning the *Ember-goose*. And am, with esteem,

Yours,

Edinburgh, March 17, 1809. PAT. NEILL.

The Danish clergyman, whose account is quoted by your correspondent, is said to affirm, that the ember-goose "lives constantly on *dry land;* and although it has been often seen with grown up young, no person has ever found its

Blunder in the Danish clergyman's account of the immer diver

its nest." There is here, in my opinion, a palpable blunder, which must have arisen either from a mistranslation, or from the accidental omission of some words. If the original were consulted, I should not be surprised to find it run thus: "The ember lives constantly at sea, and is never seen on the dry land," &c. That this must be the import, seems evident from the rest of the account. If the ember lived constantly on the dry land, in the narrow and confined islands of Feroe, the nest and young of so large and remarkable a bird must have been familiar to the natives; yet we are told, that, "although it has been often seen with grown up young, no person has ever yet found its nest." It is indeed added, "As it has a large hole under each wing, many have imagined, that it there hatches its eggs."

probably a mistranslation.

Supposing that the eggs were really hatched in hollows under the wings, (which is too extravagant a notion to be granted without complete proof) we cannot for a moment believe, that the young could remain there till they were "grown up." But further, if the ember lived constantly on dry land, there would evidently be no occasion at all for this singularity in the manner of hatching its eggs; which, on the other hand, might seem commodious, on the supposition that the bird lived constantly at sea. And the opinion, that it does live constantly at sea, has procured it sometimes the striking appellation of the "herdsman of the sea."

Improbability of the account.

If any confirmation be wanted, I may state, that, by the correction I have suggested, the Feroe account of the ember is brought to agree perfectly with the opinion entertained at this day by the common people in the Orkney and Shetland islands. These, it will be recollected, were formerly subject to the Crown of Denmark, and ultimately connected with the Feroes. That the vulgar notions, therefore, prevalent in our own northern islands and in the Feroes should coincide, is extremely natural: that they should be directly contrary to each other, seems exceedingly unnatural and improbable.

Opinion of the Orkney and Shetland islanders.

In the course of visiting many of these islands in the summer of 1804, I made frequent inquiries concerning the habits

Inquiries in these islands.

habits of the ember-goose, both of the best informed gentlemen, and of the fishermen and common people.

The common people believe it hatches its eggs under the wing.

By the latter class I was uniformly assured, that the ember continues constantly at sea, without ever touching the land; and that it hatches its eggs in holes under its wings. This last opinion I found was adopted, because, though the ember is never seen on land, nor have its nest or eggs ever been discovered in the islands, yet the old ember is frequently observed in the friths and bays, attended by a couple of young ones. I remarked that, both in the Orkney and the Shetland islands, the common people in general made no distinction between the true immer and the northern diver, but included both under the name of ember-geese: some fishermen, however, denominated the northern diver, the great emmer, or ember; but the hatching of the egg under the wing was supposed to be equally characteristic of both.

Account given by the better informed.

From the gentlemen resident in both sets of islands, who were sportsmen, or had been sportsmen in their youth, I learned, that both the true immer, colymbus immer, and the northern diver, colymbus glacialis, frequent the friths and bays during the whole year, and very much resemble each other in their habits; only the northern diver is observed to be more common in winter than in summer, while the emmer is equally common all the year round. On this account some gentlemen were of opinion, that this last might probably breed in some of the unfrequented *holms* *; but they acknowledged, that its nest had never been found: indeed neither species had ever been seen to go ashore; far less been known to breed. I was told, that when pursued by a boat, both kinds swim with astonishing velocity; when approached, they dive very rapidly; and occasionally changing their course under water, rise to the surface at a great distance, and in a quarter altogether unexpected; thus baffling the efforts of their pursuers. When suddenly surprised, or very much teazed, they sometimes, though but rarely, run along the water, beating it violently with their wings, and uttering cries not unlike the howlings of some

* A *holm* is a small uninhabited island, used only for pasture.

 small

small dogs; but they have never been observed to get fully on wing, or even to attempt an elevated flight. The young ones, which are seen accompanying them, are always, I learned, of sufficient size, to render it possible that they may have come from a great distance, perhaps Iceland, Norway, or Greenland: this is an important remark, and the testimony was uniform.

Perhaps breeds far to the north.

There is no hole, or remarkable hollow under the wing.

In regard to the alleged hole under the wing, I can assure your correspondent, that no such hole exists. I affirm this, not only from having myself examined prepared specimens of the immer, in which no trace of such a cavity existed; but on the authority of those who have shot the bird, or caught it, as sometimes happens, on a baited hook on a sunk line; and who declared, that on examination they found no greater hollow under the wing of the ember, than may be seen under the wing of the common goose. The same thing may be affirmed of the northern diver. I have at different times procured large and full grown specimens of this beautiful bird, which were found entangled in nets set in the Frith of Forth for thornback and skate, in the months of April and May; and in none of these were there any remarkable hollows under the wing.

Accounts given by various authors.

I shall close these remarks (which have already, perhaps, extended to too great a length) with some slight notice of the accounts to be found in books.

Wallace, sen.

The elder Wallace, in his History of Orkney, 1692, gravely states, that the emmer "has its nest and hatches its eggs under the water." Brand, a visitor sent to the islands by the General Assembly of the Church of Scotland, in his Description, published 1701, repeats the same story, with equal solemnity: "It hath its nest wherein it hatcheth its eggs, one or two at once, under the water at the foot of a rock, as they informed me hath been found." Sir Robert Sibbald, rather incautiously following these authors, gives a similar account. The other notion, of its hatching its eggs under the wing, is countenanced by Pontoppidan, in his History of Norway, 1751.

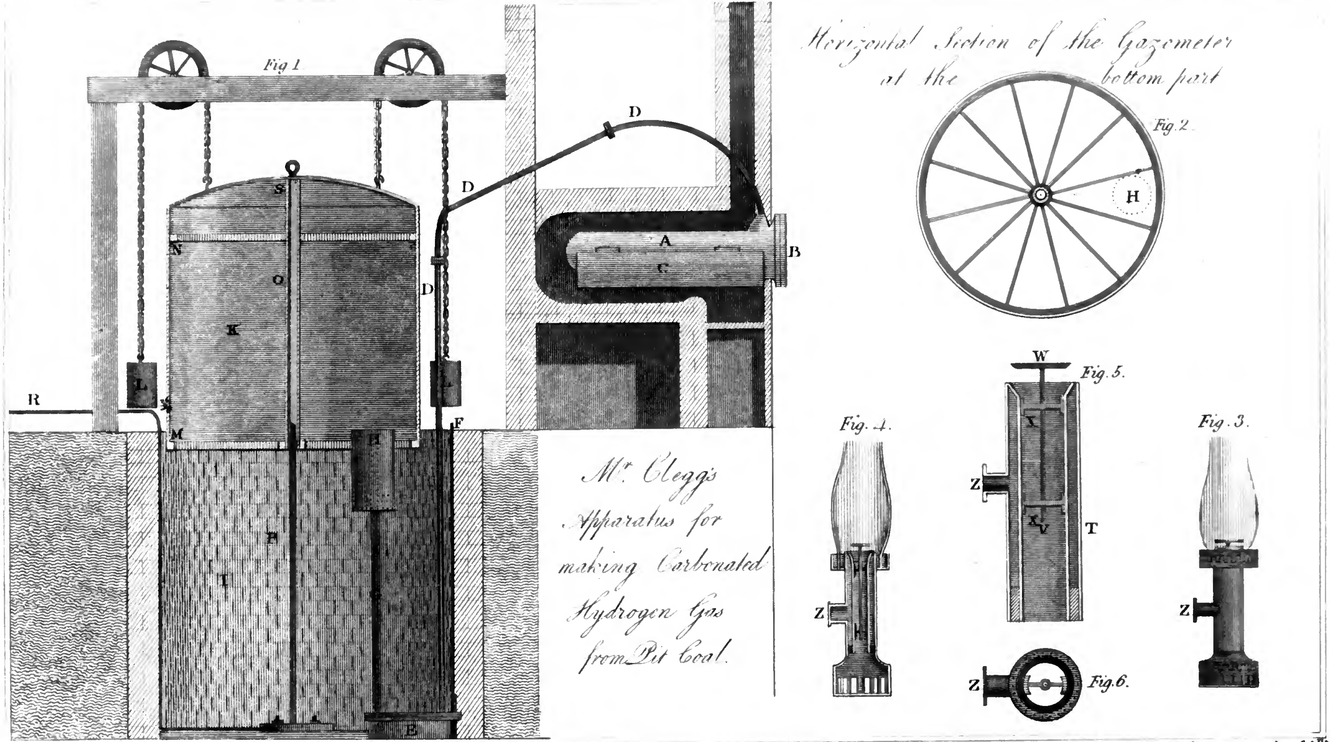
Brand.

Sir R. Sibbald.

Pontoppidan.

Horrebow.

Horrebow, however, in his Natural History of Iceland, 1758, gives a much more natural and rational account. "The *lom*," he says, "is unmolested; for the people give themselves



R. Rofieso 52 Queen Ann St. E.

themselves no trouble to look after its nest or brood, neither their flesh nor eggs being fit to be eaten. They build in remote places near fresh water;" so near, as we learn elsewhere, that the bird may almost slide into the water. It is not perhaps easy to determine, whether, by the term *lom*, Horrebow here means the proper immer, or the northern diver; but it matters not. As the habits of both are in other respects much alike, and as the breeding of the northern diver is held to be of the same mysterious nature as that of the immer, we may reasonably conclude, that both perform the offices of incubation in places of the same sort, and in a manner somewhat similar.

Upon consulting Colonel Montagu's Ornithological Dictionary, (2 vols, 8vo, 1802) a work in general of the greatest accuracy, I find, that in regard to the immer, without taking notice of any of the fabulous reports above detailed, he merely states, that "it makes a nest on the water, placed amongst the reeds and flags," in fresh water lakes. He does not, however, mention any authorities. As to the northern diver, he observes, that "it is not uncommon in Iceland and Greenland, where it breeds in the fresh waters, and is said to lay two large eggs, of a pale brown colour, in the month of June." He mentions that this bird seldom leaves the water; but that, in the spring of 1797, one was taken near Penzance in Cornwall, at some distance from water. It appeared incapable of raising itself from the ground, yet did not seem to have any defect. It lived for six weeks in a pond, eating fish thrown to it. Col. Montagu. A northern diver taken some distance from water.

II.

Description of an Apparatus for making carburetted Hidrogen Gas from Pitcoal, and lighting Manufactories with it. By Mr. Samuel Clegg, *of Manchester*.*

Dear Sir,

WHEN your son was in Manchester, he called to see my nephew, Samuel Clegg's, improved gas lights, and was Mr Clegg

* Trans. of Soc. of Arts, vol. XXVI, p. 202. The silver medal was voted to Mr. Clegg for this communication.

desirous to have a plan of his method, which my nephew promised him, and I undertook to get it conveyed to you. I have, accordingly, taken the opportunity of sending to the Society of Arts a plan and explanation of his apparatus.

used gas lights some years ago, and freed them from offensive smell.

He lighted a large manufactory in Yorkshire some years ago upon this principle, and has since lighted some buildings in this neighbourhood, and I believe he is the first person, who succeeded in rendering these lights free from the offensive smell which generally accompanies them. My nephew served an apprenticeship to Messrs. Boulton and Watt, of Birmingham, in the steam engine business, in which he is now engaged here on his own account, and has made considerable improvements in their construction.

I remain, dear Sir,

Your most obedient servant,

ASHWORTH CLEGG.

Manchester, May 18, 1808.

SIR,

Cost of the apparatus.

Your esteemed favour I have received, and, according to your request, have sent you a fuller explanation of the gasometer and lamp, accompanied with farther drawings.

A gasometer, containing seven hundred cubical feet of gas, weighs about twenty hundred weight, and costs about two pounds ten shillings the hundred weight.

The whole of an apparatus complete, capable of supporting forty lamps for four hours, each lamp affording light equal to ten candles of eight in the pound, will cost about two hundred and fifty pounds. Each lamp consumes six cubical feet of gas per hour. I am happy to find, that the Society have honoured my communications with their attention, and I remain, with great respect,

SIR,

Your most obedient servant,

S. CLEGG.

Manchester, Aug. 12, 1808.

Reference

Reference to Mr. S. Clegg's improved Apparatus for extracting Carburetted Hidrogen Gas from Pit Coal. See Plate III, figs. 1, 2, 3, 4, 5, *and* 6.

In fig. 1, *A* shows the cast iron retort, into which are put the coals intended to be decomposed by means of a fire underneath it, the heat of which surrounds every part of it, excepting the mouth or part by which the coals are introduced. The lid or iron plate *B*, which covers the mouth of the retort, is ground on air tight, and fastened by means of a screw in the centre; *C* is a shield or saddle of cast iron, to preserve the retort from being injured by the intensity of the fire underneath it, and to cause it to be heated more uniformly. *D D D* represents the cast iron pipe which conveys all the volatile products of the coal to the refrigeratory of cast iron *E*, in which the tar, &c., extracted from the coal is deposited, and whence they can be pumped out by means of the copper pipe *F*. *G* is the pipe which conveys the gas to the top of the cylindrical vessel or receiver *H*; this receiver is air tight at the top, and consequently the gas displaces the water in the vessel *H*, to a level with the small holes, where the gas is suffered to escape and rise through the water of the well *I*, into the large gasometer *K*. The use of the vessel *H* is pointed out as follows, viz. If the pipe G reached all through the water, without passing into the vessel *H*, the gas would not be rendered pure or washed; and if part of the pipe did not rise above the water, the water would have free communication with the tar, besides exposing the retort *A* to a very great pressure, so as to endanger its bursting when red hot. This vessel or receiver *H*, in a large apparatus, is about eighteen inches diameter, and two feet long; the quantity of gas, therefore, which it contains, is sufficient to fill the pipes and retort when cool, prevent the pipe *G* from acting as a siphon, and expose the gas to the water without endangering the retort.

Description of the apparatus.

When the operation begins, the upper part of the cylindrical gasometer *K*, fig. 1, made of wrought iron plates, is sunk down nearly to a level with the top of the circular well *I*, and is consequently nearly filled with water, but it rises gradually

Description of the apparatus.

gradually as the gas enters it and displaces the water; the two weights *L L* suspended over pullies by chains keep it steady and prevent its turning round, otherwise the lower stays *M* of the gasometer would come into contact with the vessel *H*. There are two sets of these stays, one shown at *M*, and the other at *N*.

There is also an iron pipe *O*, made fast in the centre of the gasometer by means of the stays, which slides over the upright pipe *P*, by which contrivance the gasometer is kept firm and steady, when out of the well; it likewise prevents the gas from getting into the cast iron pipe *P*, and the copper pipe *R*, any where but through small holes made in the pipe *O* at *S* at the top of the gasometer, where the gas is perfectly transparent and fit for use.

The pure gas enters the tube *O* at the small holes made in its top at *S*, and passes on through the tubes *P* and *R* to the lamps, where it is consumed and burnt.

The seams of the gasometer are luted to make them air tight, and the whole well painted inside and out, to preserve it from rust.

Fig. 2 shows a horizontal section of the lower hoop of the gasometer *K* at the part *M*, with its stays or arms, and the manner in which the iron pipe *O*, before described in fig. 1, sliding on the tube *P*, passes through the ring in the centre of the hoop. A horizontal section of the receiver *H* appears therein.

Lamps for burning the gas.

Fig. 5 shows a section of one of the gas lamps. The space between the outer tube *T* and the inner tube *V*, is to be filled with gas supplied by the pipe *R*, shown in fig. 1, where a stop cock is inserted for adjusting the flame, which gas passes through a number of small holes made in the outer edge of a circular plate shown at fig. 6, which unites the tubes *T* and *V* at their tops. *V* is the inner tube which conveys the atmospheric air into the centre of the flame; the upper part of this tube is made conical, or widening outwards, to join a circular plate with holes in it, a horizontal view of which is shown at fig. 6. *W* is a button, which can be placed at a small distance above the mouth of the lamp, and its use is to convey, in an expanded manner, all the air which rises through this tube to the inner surface of

of the flame, which assists the combustion very much; this button may be set at any convenient distance above the tubes of the lamp, as it slides in the cross bars *XX*, by which it is supported in the inner tube.

A current of air also passes between the glass tube or chimney and the outer tube *T*, through holes made in the bottom of the glass holder, as in Argand's lamps; this surrounds the flame, and completes its combustion, as explained by the view, fig. 3, and section, fig. 4, which have a glass upon each. *ZZZZ*, figs. 3, 4, 5, and 6, show the tube through which the lamp is supplied with gas from the pipe *R*, fig. 1.

III.

A Cheap Method of Preserving Fruit without Sugar, for Domestic Uses or Sea Stores. By Mr. THOMAS SADDINGTON, *No. 73, Lower Thames Street* *.

SIR,

I SHALL be much obliged to you to lay before the Society of Arts &c. the enclosed communication, and a box containing the following fruits in bottles, preserved without sugar, namely, apricots, gooseberries, currants, raspberries, cherries, Orleans plums, egg plums, green gages, damsons, and Siberian crabs. I have also sent some fresh English rhubarb plant, preserved in a similar manner. The same mode is applicable to other English fruits, as cranberries, barberries, and many more. This manner of preserving fruit will be found particularly useful on ship-board for sea stores, as the fruit is not likely to be injured by the motion of the ship, when the bottles are laid down on their sides, and the corks kept moist by the liquor, but on the contrary will keep well even in hot climates.

Fruits preserved by the new mode.

Applicable to others.

Particularly useful for sea.

* Trans. of the Soc. of Arts, vol. XXVI, p. 145. Five guineas were voted to Mr. Saddington for this invention.

The

and cheap. The cheapness of the process will render it deserving of the attention of all families from the highest to the lowest ranks of society. If the instructions I have sent are well attended to, I have no doubt, that whoever tries my method will find it to answer his expectation,

I am, Sir,

Your most obedient humble servant,

THOMAS SADDINGTON.

A new Method to preserve various Sorts of English Garden and Orchard Fruits, without Sugar.

Fruit generally useful, The general utility, as well as luxurious benefit, arising from the fruit produced by our gardens and orchards, is well known and acknowledged at the festive board of every family; nor is this utility and benefit less manifested by a desire of many persons to preserve them for culinary purposes in the more unbountiful season of the year; and I am well persuaded, that this commendable desire would be but preserving it expensive, greatly extended in most families, was it not attended with so much expense as is generally the case by preserving fruit in the common mode with sugar, this article chiefly constituting the basis by which it is effected. In addition to the expense of sugar, which is frequently urged as a reason for not preserving, there are other objections to that method, and what I am about to mention cannot be considered as the least, namely, the great uncertainty of success, and the sugar apt to ferment, occasioned by the strong fermentable qualities contained in many sorts of fruit. It may be said by some, that fruit may be preserved for a length of time without sugar by the ordinary mode of baking or boiling, and being closely stopped up, to which assertion I freely assent; but even this method is frequently attended with uncertainty, for if the cork or other means used for keeping the external air or the fruit grow mouldy. out of the vessel becomes dry, or from any other cause the atmospheric air exchanges place with what is impregnated by the fruit, it soon becomes mouldy and unfit for use.

These disadvantages removed. From these considerations, and a desire of preserving fruits at a trifling expense, I have made various and successful

cessful experiments of doing it without sugar, and at the same time with a certainty of their retaining all those agreeable flavours which they naturally possess; and it is highly probable, that they will keep perfectly good for two or three years, or even a longer period, in any hot climate, by which it appears to become a valuable store for shipping or exportation, as I have exposed them to the action of the meridian sun in an upper room, during the whole of the summer, after they have been so preserved (being done in 1806). I have now the pleasure of laying before the Society specimens of the fruit alluded to.

Process for preserving Fruit.

The bottles I chiefly use for small fruit, such as gooseberries, currants, cherries, and raspberries, are selected from the widest necked of those used for wine, or porter, as they are procured at a much cheaper rate than what are generally called gooseberry bottles. Having got them properly cleaned, and the fruit ready picked, (which should not be too ripe,) fill such of them as you intend doing at one time, as full as they will hold, so as to admit the cork going in, frequently shaking the fruit down whilst filling. When done, fit the corks to each bottle, and stick them lightly in, so as to be easily taken out when the fruit is sufficiently scalded, which may be done either in a copper, or large kettle, or saucepan over the fire, first putting a coarse cloth of any sort at the bottom to prevent the heat of the fire from cracking the bottles: then fill the copper, or kettle, with cold water sufficiently high for the bottles to be nearly up to the top in it: put them in sideways to expel the air contained in the cavity under the bottom of the bottle; then light the fire if the copper is used, taking care that the bottles do not touch the bottom, or sides, which will endanger their bursting; and increase the heat gradually until it comes to about one hundred and sixty, or one hundred and seventy degrees, by a brewing thermometer, which generally requires about three quarters of an hour. For want of such an instrument it may be very well managed by judging of the degree of heat by the finger, which may be known by the water feeling very hot, but not so as

Process described.

to

to scald it. If the water should be too hot, a little cold may be added to keep it of a proper temperature, or the fire may be slackened. When it arrives at a sufficient degree of heat, it must be kept at the same for about half an hour longer, which will at all times be quite enough, as a longer time, or greater heat, will crack the fruit.

During the time the bottles are increasing in heat, a tea kettle full of water must be got ready to boil as soon as the fruit is sufficiently done. If one fire only is used, the kettle containing the bottles must be removed half off the fire, when it is at the full heat required, to make room for boiling the water in the tea kettle. As soon as the fruit is properly scalded, and the water boiling, take the bottles out of the water one at a time, and fill them within an inch of the cork with the boiling water out of the tea kettle. Cork them down immediately, doing it gently, but very tight, by squeezing the cork in, but you must not shake them by driving the cork, as that will endanger the bursting of the bottles with the hot water; when they are corked, lay them down on their side, as by this means the cork keeps swelled, and prevents the air escaping out: let them lie until cold, when they may be removed to any convenient place of keeping, always observing to let them lie on their side until wanted for use. During the first month or two, after they are bottled, it will be necessary to turn the bottles a little round, once or twice in a week, to prevent the fermentation that will arise on some fruits from forming into a crust, by which proper attention, the fruit will be kept moist with the water, and no mould will ever take place. It will also be proper to turn the bottles a little round once or twice in a month afterwards.

Recapitulation. Having laid down the method of preserving fruit without sugar, in as clear and concise a manner as possible, I will recapitulate the whole in a few words, which may be easily remembered by any person. Fill the bottles quite full with fruit. Put the corks in loosely. Set them in a copper, or kettle of water. Increase the heat to scalding for about three quarters of an hour; when of a proper degree, keep at the same half an hour longer. Fill up with boiling water.

ter. Cork down tight. Lay them on their side until wanted for use.

It may be said as an additional reason, as well as cheapness, for using wine, or porter bottles, instead of gooseberry, that there is a difficulty of obtaining them, even at any price, in some parts of the country; and indeed they are equally useful for small fruit, and answer the purpose quite as well, excepting the little inconvenience of getting the fruit out when wanted for use, which may be easily done by first pouring all the liquor out into a bason, or any other vessel, and then with a bit of bent wire, or small iron meat skewer, the fruit may be raked out. Some of the liquor first poured off serves to put into the pies, tarts, or puddings, instead of water, as it is strongly impregnated with the virtues of the fruit, and the remainder may be boiled up with a little sugar, which makes a very rich and agreeable syrup. Bottles.

In confirmation of the foregoing assertions, I now produce twenty-four bottles as samples, containing twelve different sorts of fruit, viz. apricots, rhubarb, gooseberries, currants, raspberries, cherries, plums, Orleans plums, egg plums, damsons, Siberian crabs *, and green gages—which have all been preserved in the manner above described. Specimens.

In order to diversify the degree of heat, and time of continuance over the fire, I have done some in one hundred and ninety degrees, and continued them in it for three quarters of an hour; from which experiments it is evident, that the heat is too powerful, and the time too long, as the fruit by this degree and continuance is rendered nearly to a pulp †. The heat must not be too great, or too long continued.

In the summer of 1807 I preserved ninety-five bottles of fruit, the expense of which, (exclusive of bottles and corks) was £1 9*s.* 5½*d.*; but having some fruit left, it will not be right to judge them at a higher rate than £1 9*s.*; and allowing 5*s.* for the extra coals consumed in consequence of Cost.

* Apples and pears may be done for shipping, &c.

† Some of these samples of 1807, were done in 180 and 190 degrees.

my

my not having a conveniency of doing more than seven or eight at a time, and this being done at fourteen different times, it will amount to £1 14*s.*; the average cost of which is nearly 4½*d.* per bottle, exclusive of the trouble of attending them. But if we estimate their value in the winter season at 1*s.* the bottle, this being in general as low or lower than the market price, they will produce £4 15*s.*; but losing one bottle by accident, reduces it to £4 14*s.*, leaving a net profit of £3 on ninety-four bottles, being a clear gain of nearly two hundred per cent.

Profit.

For ship's stores.

Another great advantage resulting from this statement will appear by making it an article of store for shipping, or exportation; and I shall submit a few ideas tending to promote such a beneficial object by doing it in large quantities; for which purpose sufficiently extensive premises must be fitted up, with a proper number of shelves, one above another, at a distance of about five inches.

Method of doing it on a large scale.

The vessel for scalding the fruit in should be a long wooden trough of six, eight, or ten feet in length, two or three in breadth, and one in depth, fitted with laths across to keep the bottles upright, and from falling against one another; this trough of water to have the heat communicated to it by steam, through a pipe from a closed boiler at a little distance. The boiling water, wanted to fill the bottles with, may be conveyed through a pipe and cock over the trough, by which arrangement, many hundreds of bottles might be done in a short time. It may be prudent to observe, that this idea is only speculative, not having been actually practised, but at the same time seems to carry with it a great probability of success, and worthy the experiment.

It remains now, that I state some reason or object for troubling the Society, whom I have taken the liberty to address with these communications. The first is a desire of publicity, sanctioned by their investigation of the experiments made for preserving fruit without sugar, thereby lessening the expense attending an object of so much public benefit and utility. The second arises from a personal or private consideration; but on this subject I shall only observe, that I wish to throw myself entirely on that protection

tection which has ever characterised the liberality of the Society; and that I shall feel highly honoured, if they conceive what I have communicated deserving any mark of their favour.

I am, Gentlemen,

Your most obedient humble servant,

THOMAS SADDINGTON.

IV.

On Reclaiming Waste Lands. By Mr. WAGSTAFFE *.

GENTLEMEN, *Norwich, June 27,* 1801.

AS your influence for the enclosure of Waste Land is confessed, and, I conceive, extending within the scope of your Society, and it should now seem on the eve of a Parliamentary encouragement; I ask leave to recite an experiment I made on a portion of land, of as obvious ſterility as perhaps any present waste within the Western counties. Waste land

This was an acclivity, which had not been cultivated within memory; and at the foot of it a various tract, gravelly and moory, broken into hollow spaces, in which waters rested during the summer months, which waters were covered with most of the aquatic plants native to stagnant pools. My predecessor in possession of these watery wastes, during a summer drought, fed their interstices with sheep, which became diseased, and many of them rotten. described.

The mode I pursued was as much as might be to extract the weeds, roots, and sediment; lay them in heaps as a preparation of manure measurably to replace and fertilize the barren sands and gravel, brought from the heights to fill up these hollows. I then opened ditches, raised their sides with sand and gravel, and on them planted large cuttings of poplars and willows. The ditching drained the soil, and the materials from the heights raised this swamp Steps taken to improve it.

* Bath Papers, vol. X, p. 18.

Fence.

to the proper condition of meadow. The upland I enclosed with thorns on a willow ley *, and within the banks inlaid them with seedling trees and forest; divers of the former have been taken down for use, and some of the aquatic cuttings are grown to a timber measure; while the several subdivisions, meadow and upland, have been cultivated, and borne every species of grain and herbage, confessedly upon an equality with the long tillaged circumjacent fields.

The process applicable to great extent.

By a process thus pursued, of which I have presumed to adduce this example, the numerous millions of waste acres, which yet disfigure our nation, may and will become, the seasons favouring, under your and your compatriots' encouragement, a widely extended garden, replete with every useful production congenial to our climate; and the boundary of its fields fenced with faster thriving trees, and more abundant in number than the present large tracts of forest produce, provide for generations yet to come an increase of those necessary timbers, that have given this island an intercourse with the inhabitants of every maritime clime, and an acknowledged superiority in the commercial world, which probably it would not have obtained but from the indigenous growth of these not sufficiently valued timbers.

Timber.

Although your extended encouragements have much increased them by multiplied plantations, yet their growth may be indefinitely enlarged by an encouragement for their acorn seed to be placed in every raised bank, or their seedlings planted in every new formed hedge-row; which most efficaciously might be enforced by Parliament as a conditional obligation on all to whom they are assigned, under the statute of a national enclosure. But as every seminary of oaks must be referable to a distant posterity, it becomes worthy of every present planter in the interior of his hedge-rows to have large cuttings of poplar * and willow,

* A willow fence in this situation has the appearance of improbability, but it is yet improving.

Different poplars.

† Of poplars, the *nigra*, *alba*, and *hybridum*; this latter hath not, I conceive, found its way into any systematical arrangement of plants, and in course has not received any specific character. The name assigned

willow *, and an intermixture with young trees of the resinous tribe. Those I have already known may be taken down as timber during the life of the planter, and as early as the inlays are grown to afford shelter and shade to the herd and the flock, that occasionally feed within their enclosures.

I may just add, the fall of the autumnal leaf, with the manure of the depasturing cattle, may continue the fertility of these fields without extraneous aid; and where not readily procurable, I may farther add, that in the latter end of the autumn of 1799 I procured turves from different wastes, reserved them on a gravel walk, and thereon dibbled wheat, almost every grain of which succeeded, branched into divers stems, which severally bore a full and perfect grain. In the autumn of 1800 † I repeated the trial, which at this instant is as promising as the other proved. The early spring of this year, 1801, I practised the same mode with tares, pease, oats, and barley, which severally are promising. I bring forward these experiments to show, that generally every waste may be rendered productive by the first simple operation of the plough, and thereby supersede the long process pursued by many; call forth to the earliest production the unprofitable wastes of the kingdom; and hence, as far as human foresight can discover, prevent such a sensible

The soil of various wastes not unsuited to grain.

signed it is on the opinion of a gentleman well acquainted with botanic distinction, who conceives it to be a variety, perhaps of the two former. I may speak from an enlarging experience, that it is a handsome and fast growing tree, multiplies itself distinctly from its roots, while its cuttings take with nearly equal facility as the two former.

* *Pendandria*, (laurel leaved) *amygdalina*, (almond leaf) *alba*, (common gray leaf.) These three species I know, or presume, on the progress the first has already made, will severally grow to a timber bulk. The prospective diversity of contrasted foliage can perhaps be not better exemplified than in the vivid green of the laurel willow, and the hoary leaf of the white poplar.

Willows for timber.

† There is an average of four large ears to every grain dibbled, now in full flower, which conveys an expectation of more than a hundred fold increase, the actual increase of the preceding year. These turves or flags have received no aid from manure, or any artificial watering.

 scarcity

scarcity as most of our provinces have recently felt. And again, under the blessing of Providence, witness a competency for ourselves, and a surplus for other nations; and thence be commercially beneficial to a large portion of mankind.

I am, with sincere regard,

Your respectful friend,

JOHN WAGSTAFFE.

V.

Account of Waste Land improved by J. BUTLER, *Esq. of Bramshott, in Hampshire*.*

SIR,

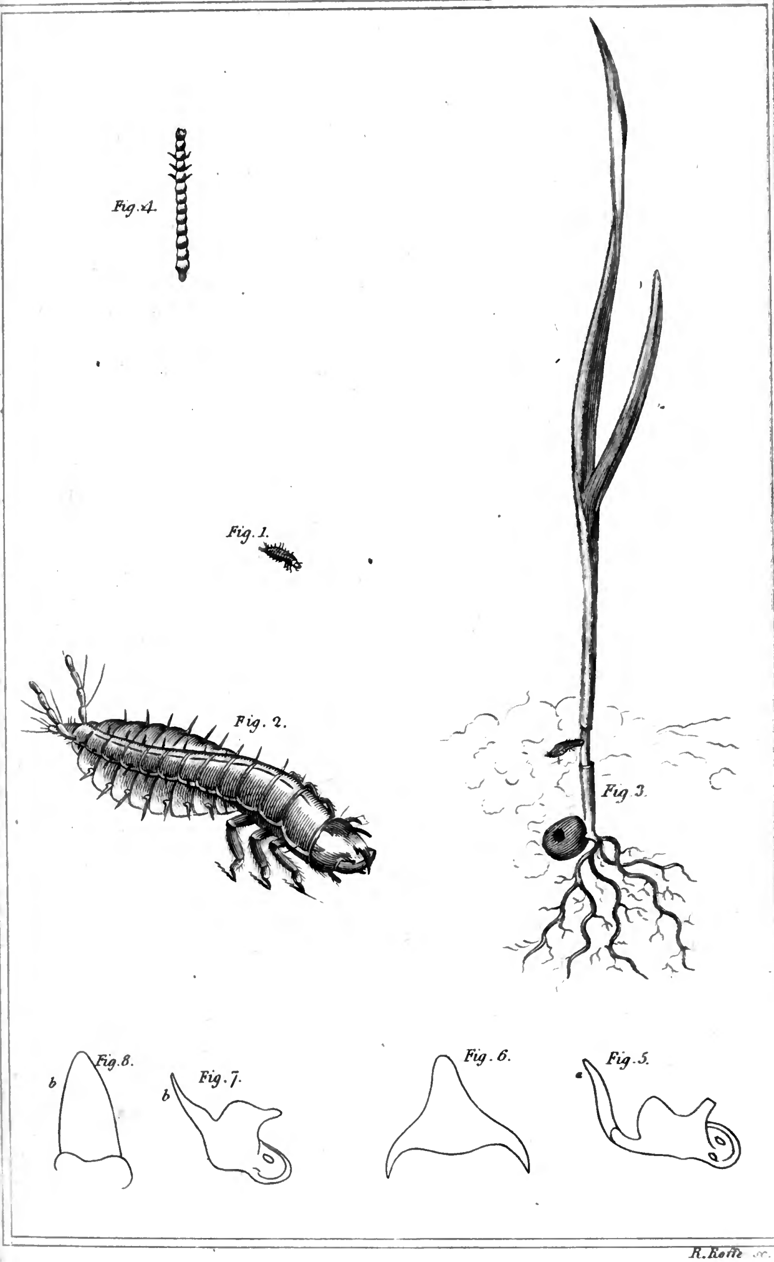
Waste land. IN the year 1802 I purchased an estate, situate in the parish of Bramshott, in the county of Hants, of which seventy acres and upwards were then waste lands, growing a little timber, furze, and alder, and supporting a few cows in the summer, but never cultivated or considered worth that expense.

General state. From particular engagements at the time, I did not begin any improvement till 1804, when I found sixty-five acres and a half (statute) of the said waste lands in the following state: twelve acres, the site of old fish ponds, growing nothing but reeds and rubbish; eighteen acres one rood thirty-seven perches, affording a little sour grass and a few alders in wet places; twenty-seven acres three roods one perch, quite a morass or bog, with a few alders; and seven acres one rood four perches of very indifferent furze.

First drained. As the greatest part of the waste was filled with innumerable springs that deluged the whole, and caused the bog to be saturated throughout the year, I considered that

* Trans. of the Soc. of Arts, vol. XXVI, p. 117. The silver medal of the society was voted to Mr. Butler.

draining



R. Roffe sc.

draining was the first necessary step to be taken. With this view I made an open cut or ditch, of five to seven feet deep or more, in the lowest part of the bog, to let out the stagnated water, and ascertain with precision the cause that produced it. Having obtained the lowest possible fall by the open cut or ditch, I caused other cuts to be made to the heads of different springs which fed the land, occasionally boring with the auger, that no spring might be passed over; and I then laid the open cuts or drains with stones from three to twelve feet below the surface, according to circumstances, to carry off the water, observing always to keep the level.

At the highest ground I found rocks, under which the principal springs lay at the distance of at least fifteen feet, and thence an immensity of water gushed out, which was easily passed off through the drains, and I had the satisfaction to find, that in the course of two years the whole waste became perfectly dry, and so continued. Principal springs.

The extent of land thus drained being great, the cost of course is very considerable, and amounts to the sum of £338 2*s.* 11*d.* Cost.

During this I grubbed the greatest part of the land, which from the stems of oak and other timber that had formerly grown there, as well as the alder, furze, and some timber standing at the time I made the purchase, was no inconsiderable work, and cost for each oak stem ninepence, and for the soil on which the alders and furze grew, sixpence per rod, amounting in the whole to the sum of £95. Expense of grubbing.

The ground being now cleared, I ascertained, by the means of a water level, the position of a little brook which ran through the waste land, and found that it was practicable to turn water over thirty acres of it. This being an object of the first consequence, I spared neither pains nor expense to accomplish it. I removed the high banks round the fish ponds, which contained some thousand loads of soil; filled up very deep ditches and stew ponds, and laid several acres on an inclined plane; burnt the roots and rubbish, and prepared, by levelling and making water carriages, thirty acres for irrigation, of which sixteen acres, Prepared for irrigation.

though rough and some pasture as before described, had by draining and feeding began to improve considerably; and in the spring of 1806, I was enabled to turn the water over such sixteen acres, from which I derived a tolerable crop of hay in that summer. Feeding it harder afterward, and watering it the following winter, there was a good supply of feed for sheep till the latter end of April; when it was laid up and watered as before, but with far better success, as the crop was not only greatly improved in quality, but likewise in quantity, producing more than two tons to an acre throughout the sixteen acres.

The residue of the thirty acres prepared for irrigation as before stated, formerly fish ponds and other rough lands, but lately levelled and sown with perpetual grasses, is now looking remarkably well, and will certainly be in readiness to receive the water, as soon as the land is firm enough for this purpose.

Cost.

In accomplishing this work I had the assistance of Joseph Trigger, who lived with and managed the water meadows of the late Mr. Bakewell of Dishley for more than twenty years; and it would be an act of injustice to him not to say, that the said land is prepared for a water meadow in a masterly style. This cost me not less than £223.

Pared and burned.

As soon as I perceived the effect of my drains on the bog, which was composed of a good deep peat, I pared by hand thirteen acres of it, which I burnt, and spread the ashes; then ploughed the land once, and sowed it with cole seed in the month of July. The crop turned out very good, and fed one hundred and sixty two-shear hogs (Leicester) for two months; after which I ploughed it once only, and sowed it with white oats in the month of April 1807. At first the oats appeared sickly, but receiving a few warm showers in May, they recovered and flourished exceedingly, making a most excellent appearance, to the astonishment of the neighbourhood; for when reaped, they were estimated at from ten to thirteen quarters per acre, some parts being preferable to others, but the whole good; and I have no doubt, for at present they are not threshed, that the crop will amount to its estimate.

Sowed cole-seed, and next white oats.

In the course of last year I pared seven acres more of the said

said bog, and burnt and spread the ashes in a similar way to the former, and sowed it with cole seed from one ploughing in July last, which likewise turned out a most excellent crop, and supported seventy-two large sheep on it for more than two months.

The expense of paring and burning the twenty acres came to £29 10*s.* Expense.

The remaining waste land being a lighter peat, mixed more with sand, I did not think it advisable to pare and burn, but contented myself with fallowing it for turnips, with which it was sown last summer; but from the indifference of the season, the crop did not prove abundant, yet as much so as I had any reason to expect; and I have no doubt, by proper management of it, though by far the worst of the waste, it will shortly become very useful land, and produce in succession good turnips, barley, and seeds. Part fallowed for turnips.

On a review of the foregoing statement, it will appear that the expense attending the improvement of the waste has been great; but it will be recollected, that the quantity of land reclaimed is very considerable, the greatest part of which has been drained and grubbed, and the face of it entirely changed; that on the comparison I now submit, I feel great satisfaction in being enabled to assert, in the judgment of able men, that at the time I made the purchase, the waste land was not worth more than 5*s.* per acre per annum on an average, which amounts to £16 7*s.* 6*d.* and that it is now worth and let as follows: General statement of expense & profit.

	£.	s.	d.
Sixteen acres of water meadow, £3 per acre -	48	0	0
Fourteen ditto will shortly be as valuable - -	42	0	0
Twenty ditto of reclaimed bog, £2 per acre -	40	0	0
Fifteen and a half ditto lighter peat, £1 per acre	15	10	0
By the year - - - - - - - - -	£145	10	0

I have not pointed out minutely every step that has been taken to drain, to irrigate, or improve the said waste lands, because the subject is generally so well understood; but I trust I have stated sufficient to prove, that the soil, thus reclaimed, is turned to a great and lasting benefit.

J. BUTLER.

VI.

VI.

Some Observations on an Insect that destroys the Wheat, supposed to be the Wireworm. By Thomas Walford, *Esq, F. A. S. & L. S.* *With an additional Note,* by Thomas Marsham, *Esq. Treas. L. S.* *

The wireworm not known.

THE insect which is the subject of the following memoir has never, I believe, been noticed or described by any entomologist or agriculturist; its depredations are the annual topic of conversation with the latter, yet few know what insect it is, that destroys the wheat in the months of October and November, under the denomination of the wireworm. Many suppose it to be a *scolopendra*, others a species of *iulus*, and some the larva of a *tipula*, or of the *scarabæus melolontha* of Linnæus. I supposed it to be one of the above, till I found two insects in the very act of destroying the wheat, as represented in the annexed figure (Pl. IV. fig. 3, *a*.). These I believe to be the insects commonly, although very improperly, called the wireworms in Essex and Suffolk: they appear to me *larvæ* of one of the coleopterous tribe; but to what genus they belong can at present only be conjectured. The projecting jaws somewhat resemble those of a *lucanus*. The two jointed bristles, and the cylindrical tail, give it an affinity to *staphylinus*; but the *larva* of this insect is supposed to be carnivorous, and not graminivorous. I fear, therefore, that the genus of this insect cannot be determined, till it is traced to its perfect state.

Larvæ of a coleopterous insect.

I shall now proceed to relate the discovery of the insect, and to detail the injury supposed to be done by it.

Discovery of the insect.

In October 1802, having occasion to call upon an agriculturist †, whose skill and judgment in farming are rarely equalled, he informed me, that his green wheat was dying and losing plant very much, the reason of which he could not comprehend. I immediately suspected, that it was occasioned by the wireworm; but what kind of insect it was,

* Trans. of the Linnean Society, vol. IX, p 156.

† Mr. Thomas Olley, of Stoke next Clare, in Suffolk.

I could

I could not inform him. I therefore requested, that he would accompany me to the field where the greatest injury was done, in order that we might examine into it. This we accordingly did; and we were successful in discovering three of the insects in question, of which two were in the act of destroying the wheat, as above mentioned. With their projecting jaws these insects cut round the outside grass about an inch below the surface of the soil, to get at the young white shoot in the centre, which they eat: upon this, vegetation is immediately ſtopped, and the plant dies. I suspect, that they first eat the flour in the grains which has not been drawn up by vegetation; for, when we touched them, they ran into the husks; and two of the three insects I carried home in the husks, which appear to be their habitations, and probably the place where they change from the *larva* to their present ſtate.

Its manner of destroying wheat.

The injury which the public sustains by the ravages of these insects may, in some measure, be calculated from Mr. Olley's loss in 1802: he sowed fifty acres of a clay soil with wheat; out of these ten were destroyed by them, which were replanted by dibbling in one bushel of seed per acre. The price of wheat at that time was eight shillings per bushel.

Great injury done by it.

We here observe one fifth part of the quantity sown destroyed by theſe noxious insects, but the depredations of the wireworm, as I am informed by a friend* whose experience and observation enable him to calculate with superior judgment, being principally confined to wheat sown upon clover leys, old pastures recently broken up, pea and bean stubbles, &c., we may suppose the general average of the injury to amount to much less than a fifth (Mr. Olley's loss): a twentieth part of what is sown upon this description of lands will, I think, be deemed a very fair and moderate calculation. The number of cultivated acres of land in England at the time above mentioned was computed at seven millions, of which 2,400,000 were calculated to be sown with wheat; and as only one half of the wheat annually sown is supposed to be upon clover leys, old pas-

Calculation of annual loss to the kingdom from it.

* Allen Taylor, Esq. Wimbish-hall, Essex.

tures

tures, &c., our calculations muſt be confined to 1,200,000 acres instead of 2,400,000: this will give 60,000 acres as annually destroyed by the insect in queſtion; which replanted, at one bushel per acre, will require 60,000 bushels of seed, which, at eight shillings per bushel, are worth £.24,000. Beside this, although no extra expense is incurred by the farmer in preparing the land, yet he has to pay for dibbling in the seed, which, at five shillings and threepence per acre, will cost £.15,750, or, at the full price, six shillings per acre, £.18,000. If the land requires harrowing, there will be a further charge of ninepence per acre, or £2,250, not to name other items, which render it difficult precisely to ascertain the loss of the farmer.

If the above calculation be thought a fair one, and I see no reason why it should not, we find the quantity of wheat lessened to the market by the depredations of these insects is very frequently, if not annually, ſixty thousand bushels; which occaſions to the farmers an additional expense of at leaſt £.15,750.

Means of preventing the injury to be sought after.

I hope these observations will prove a spur to gentlemen more conversant in entomology and agriculture than myself, to excite them to inquire into this subject, the result of which must ultimately be beneficial to the public at large, by discovering some means of preventing the iujury done by these mischievous insects. At present we know of no other than early ploughing, which is not always convenient to the farmer, as he wants to feed his clover land as late as the season will admit of. Unslacked lime has been tried without success*; although it is well known, if laid thick upon the land and ploughed in immediately, it will deſtroy insects of every kind, that are in the soil; but in many places the expense of procuring lime is too great to think of uſing it in sufficient quantities to answer the intended purpose†.

Early ploughing not always convenient.

Lime ineffectual.

As

* Farmer's Magazine, page 450.

Grub of the tipula.

† I am aware of its being said that part of the injury sustained is done by the grub of the tipula or crane-fly; but I beg leave to observe, that the injury done by the grub is in the spring, and not in October; as

As the drawing is from the accurate pencil of Mr. Sowerby, no description of the insect is necessary.

Explanation of the Figures.

Plate IV. Fig. 1. The insect, natural size.
2. The same, magnified.
3. *a.* The same, deftroying the wheat.
— *b.* Hole in the husk, into which the insect ran upon being disturbed.

Additional Note, by Mr. Marsham.

True wire-worm,

THE above described larva is quite new to me, nor can I find any thing like it in the various authors I have consulted, who have written on the larvæ of insects. I am therefore ignorant to which order it belongs. The name of wireworm seems to be given to various species of larvæ, but what I consider to be the true wireworm was sent to me some time ago by the right honourable Sir Joseph Banks. A figure of this I have added to the plate (Pl. IV, Fig. 4.). The history of this animal I found fully detailed in the Stockholm Transactions for the year 1777, by Mr. Clas Bierkander, vicar of Gothene, near Skarra, under the appellation of root-worm. This larva, when full grown, is about seven lines long, very narrow, of a yellow colour, shining, and very hard: the head is brown, with the extremities of the jaws black. The body is composed of twelve joints, on the last of which are two black indented specks. It has six scaly feet on the fore part of the body. Mr. Bierkander observes, that it remains five years in this state before it changes into a pupa, whence issues *elater segetis* of Linnæus. I have frequently found it both in fields and

in Sweden rootworm,

a larva

of a species of springing beetle, or skipper.

as many of the flies have not deposited their eggs till the latter end of September, and those that are deposited earlier are few of them hatched before the spring, as was proved by Mr. Strickney, whose pamphlet, entitled "*Observations respecting the Grub*," is now before me: therefore the depredations of the grub cannot be greatly prior to that time: besides, they are most plentiful in the fly state at the end of September and beginning of October.

gardens at the roots of divers plants, but never succeeded in bringing it to perfection. The author above mentioned describes four other species of root-worms; viz. *musca segetis, musca hordei, phalæna turca,* and *tipula oleracea.*

I flatter myself, that this valuable Essay of Mr. Walford's will stimulate other gentlemen who reside in the country, and who are so materially interested, to enter seriously into a minute examination of the various causes, by which grain is so frequently destroyed; so that, by a number of such inquiries and communications, we may at length be enabled to point out a remedy—as every grain of corn that can be preserved in times like the present must be a public benefit.

Mr. Bierkander's papers on the different root-worms I got translated by a friend; and the translation, with some remarks of my own, was some time since presented to the Board of Agriculture.

THOMAS MARSHAM.

VII.

An Account of the larger and lesser Species of Horse-shoe Bats, proving them to be distinct; together with a Description of Vespertilio Barbastellus, taken in the South of Devonshire. By George Montagu, *Esq. F.L.S.**

Supposed two varieties of the horse-shoe bat.

MOST naturalists have conceived an opinion, that there are two varieties of the Horse-shoe bat, *vespertilio ferrum-equinum* distinguished only by their size; as such, Gmelin quotes the *major* and *minor* of Schreber.

Larger described.

The larger species only has hitherto been noticed in England. This was originally discovered by Doctor Latham, who communicated it to Mr. Pennant, and he first made it public in his *British Zoology,* where he states it to be found in the salt-petre houses belonging to the powder mills at Dartford, frequenting those places in the evening for the sake of gnats; and also observed during winter in a

* Linnean Trans. vol. IX, p. 162.

torpid

torpid state, clinging to the roof. It is described thus: "The length from the nose to the tip of the tail is three inches and a half: the extent fourteen. At the end of the nose is an upright membrane in the form of a horse-shoe. Ears large, broad at their base, inclining backwards, but want the little or internal ear. The colour of the upper part of the body is a deep cinereous; of the lower whitifh."

Doctor Shaw, in his *General Zoology*, has nearly followed Mr. Pennant, but adds, "There is said to be a greater and smaller variety; perhaps the male and female: the greater is above three inches and a half long from the nose to the tip of the tail; the extent of the wings above fourteen."

With respect to the smaller horse-shoe bat, nothing more appears to be known than that it is inferior in size, but in other respects similar; from which may be inferred, that it is very little known, and it has not, to my knowledge, been recorded as indigenous to England. It is therefore with no small degree of satisfaction I have to announce, that it is by no means uncommon in particular situations; and I have the pleasure of congratulating the zoologist, that fortunate circumftances have enabled me to put the long unsettled opinion with respect to these two bats beyond all possible doubt; having lately taken a considerable number of both species, in each of which the sexual distinction was evident. But to render the subject more clear and incontrovertible, I shall proceed, by giving a description of the lesser species, and endeavour clearly to define the characteristic distinction between these two very analogous animals. In order, however, to prevent future confusion, I propose that the least of these should be called *vespertilio minutus*, leaving the other in full possesion of the original Linnæan trivial name of *ferrum-equinum*.

Smaller.

A distinct species.

Vespertilio minutus,

Length scarcely two inches and three quarters from the tip of the nose to the end of the tail, of which the latter is full three fourths of an inch: extent of the wings nine inches

Described.

ches and a half: weight from one dram three grains, to one dram twenty grains.

The colour above is pale rufous brown, most rufous on the upper part of the head: the nose is surrounded on the top with a broad membrane somewhat in form of a horseshoe; within this is a smaller, in which the noſtrils are placed; between these are two other small membranes standing a little obliquely, and appearing as valves to the nostrils; behind these stands a much more elevated longitudinal membrane; and further back is another transversely placed, of a pyramidal shape, standing erect behind the eyes; these last are covered slightly with hair, and some long bristles: round the upper lip under the exterior membrane of the nose is a row of minute tubercles, each furnished with a small bristle, equally well calculated to guide the lesser winged insects to the mouth, as the *vibrissæ pectinatæ* observed in several species of birds: the eyes are very small, black, and hidden in the fur: the ears large, pointed, and turned a little back at their tips; their base almost surrounds the opening, but at the outer part in each is a notch, which admits of the fore part of the ear closing within the other as a substitute for a valve so common in most other species, but of which this is destitute.

Found in Wiltshire.

It is now many years since I first noticed this species of bat in Wiltshire; once, in particular, I recollect to have seen a great many taken in the winter over the hollow of a baker's oven, having got in through a small external fissure. In the year 1804, about the latter end of the month of May, I observed several in an old building at the verge of a wood at Lackham, in the same county, erected for the shelter of cattle. In this shaded dark abode, surrounded by lofty oaks, it is not unusual to see several adhering to the plastered roof by their hind claws; and when approached, generally crawling a little to one side, and showing signs of uneasiness by moving their heads about in various directions, but not seeming inclined to take flight, till they have been repeatedly disturbed.

At this time I had not been fortunate enough to discover the haunt of *vespertilio ferrum-equinum*; but my wishes have since been amply gratified, by taking nine of the

v. ferrum-

v. ferrum-equinum, and seven of the *minutus*, many of which were conveyed home alive: of the former there were four males and five females; of the latter five males and two females. Of the *v. ferrum-equinum* the largest and smallest were both females, one preponderating four drams and a half, the other not exceeding four drams. The length of these to the setting on of the tail two inches and a half; to the end of the tail three inches and three quarters: the expansion of the wings about fourteen inches and a half. The two species differ in size,

In colour these two species are perfectly similar, except in some instances the sides and breast of the *v. ferrum-equinum* are more of a ferruginous-brown. scarcely in colour,

With respect to the face, which is so extremely curious, there appears on a cursory view scarcely a perceptible difference, except that the upper lip of the *v. ferrum-equinum* is much more tumid; but the most material distinction is in the formation of the nasal membranes, especially that which is posterior and transverse. To explain this no words can convey what a simple outline will, and therefore the curious are referred to Pl. IV, *fig.* 5, which represents the side view of the membranes of *v. ferrum-equinum*, of which *a* is the posterior tlanverse one; the front is seen at *fig.* 6. The same views are given of the nasal membrane of *v. minutus* at *fig.* 7 and 8, where *b b* represent the membranes in different points of view. In these a very striking difference is observable, and it will also be perceived, that the anterior longitudinal membrane is by no means similar in both species. but chiefly in the nasal membranes.

With respect to the teeth, it will be observed, that the *v. ferrum-equinum* possesses two minute distant fore teeth in the upper jaw, which are not to be found in the *v. minutus*; a circumstance that seems to have escaped most naturalists, this genus being usually placed in the division destitute of upper fore teeth: the canine teeth are also much stronger in proportion in *v. ferrum-equinum* than in the other species. Teeth.

Linnæus, when he placed the bats in the first order of *mammalia*, doubtless considered the whole genus to agree in possessing two pectoral teats, and no others; and this opinion Linnæus placed the bats in the first order of mammalia,

nion seems to have been confirmed by succeeding naturalists as far as treading in the path of so great a physiologist may be considered as a proof of the fact. It must, however, be acknowledged, that we should do well, if, at the same time we admire the wisdom and consummate skill of others, we were to recollect, that circumstances do not always concur to throw all the light upon a subject that might be desired, and that the wisest and most skilful philosopher is not proof against mortal fallibility.

Those who are in the habit of searching minutely into the secrets of nature well know how necessary it is to be cautious in admitting of general rules.

That the appearance of two pectoral teats in the bat genus, without any others contiguous, should lead to a conviction, that they were the only papillæ such animals possessed, may easily be conceived; but chance frequently develops what the most scrutinizing eye has sought for in vain.

but the less horse-shoe bat has two abdominal papillæ.

While I was searching for some curious insects, which were observed to move with unusual celerity amongst the fur of these bats*, the pectoral papillæ of one of the *v. minutus* were very conspicuous by the space round them being bare, as if the animal had recently suckled its young; and to my utter astonishment, on turning the fur over in every direction, I discovered two other teats very near together, situate on the lowest part of the abdomen, close to the *pubis*. It may readily be imagined, that so unexpected a discovery scarcely admitted the senses to determine the validity of ocular demonstration: the aid, however, of glasses left no doubt of the fact, and a scientific friend confirmed my opinion. At the moment of this discovery I had embowelled all the specimens of *v. ferrum-equinum*, and consequently cannot determine whether they are similarly formed or not; nor have I since procured a female bat of any other species to examine, so that it yet remains to be ascertained, whether this structure is peculiar to one or more species, or that the two abdominal papillæ are really essential to the generic character of these animals,

Whether this be a character of the genus, or peculiar to a species, not yet ascertained.

* *Celeripes vespertilionis*, a newly discovered insect.

but

but hitherto overlooked, by being so far removed from the others. On future observation must depend the place to which the bats should be properly consigned in the systematic arrangement of quadrupeds. If some species only are found to possess four papillæ, it would be a very considerable violence to nature to divide them on this account: and yet to retain them undivided in the order of *primates*, according to the Linnæan definition, would be inconsistent: but on this part of the subject there is no necessity of enlarging until we become more enlightened.

It is probable the papillæ of all the smaller bats are so contracted, except at the time of administering nourishment to their young, that they are not discoverable with the utmost attention, for even in the *v. ferrum-equinum* no pectoral teats were to be discovered, although the sexual distinction was sufficiently evident. But this very contracted state of these parts, when nature has no demand for the use assigned to them, is not peculiar to these volant quadrupeds, since we find the same difficulty in discovering them in mice.

Teats not easily discoverable except when suckling.

These bats were taken in a large cavern near Torquay in Devonshire, commonly known by the appellation of Kents-hole, and where both species are usually observed in considerable abundance clinging to the vaulted roof of the interior apartments. This vast cavern was explored with a view to obtain whatever species of *vespertilio* might inhabit it, and with expectation of procuring specimens of *v. barbastellus*, and possibly some new species, having been informed the cave abounded in number and variety. Strange, however, as it may appear, not a single instance occurred of any other species becoming an inhabitant of this dark and frightful region.

The two species found in the same place, without any other.

It should therefore appear, that these two bats are as congenial in their animal temperature, as they are similar in habit; and that in constitution they essentially differ from all the other British species.

It is well known, that all places impervious to light, and destitute of a free circulation of air, can neither be suddenly heated nor suddenly cooled by the changes of atmospheric temperature, and that the vicissitudes of such a climate

Resort to caverns from aversion to any change of temperature.

mate are extremely small: thus these species from instinct seek those dark and dreary abodes, and wholly retire from the face of day, their feelings being repugnant to the benign influence of the solar rays, which vivifies and reanimates all nature besides.

Others only shun the extremes.

The *v. noctula, murinus, auritus,* and probably *barbastellus*, whose constitutions appear more robust, do not retire into total darkness, nor wholly remove from the vicissitudes of the surrounding atmosphere; but, being formed by nature to bear a greater degree of either heat or cold, content themselves with such a hybernaculum as is sufficient to protect them equally from the extremes of one or the other. Thus we find these in the fissures of old buildings, in towers, under the eaves of houses and churches, and in the hollows of trees, and not unfrequently congregated; but they seldom or never enter those gloomy regions, which nature has consigned to the others as an exclusive right of inheritance.

The bat superior to most birds in powers of flight.

Contemplating the frolics and evolutions of these little creatures in our summer evenings perambulations must bring to recollection the extraordinary opinion of some philosophers, who scarcely admit their progressive motion to be an act of flying. How little can such have attentively observed their sudden and rapid turns in pursuit of flies! It might be fairly asked, How much inferior are the aërial excursions of a bat to that of a swallow, one of the most powerful on wing of the feathered tribe? and might we not pronounce, without risk of refutation, that a bat far surpasses the greater part of birds in its powers of flight?

Supposed not to require vision.

If we are to give the utmost credit to the experiments of Spallanzani and Mr. de Jurine, the conclusion would be, that vision is not of any apparent use to these animals, since they fly about with as much ease, and equally avoid obstacles, when their eyes are covered, or even put out, as they do previous to this operation. That their eyes, being minutely small, are not calculated to admit many rays of light, as in most nocturnal birds, must be allowed, but then they have no occasion to distinguish their prey at a distance. If it be denied, that their eyes are of any use in

the

in the discerning of objects against which they might strike, surely they must be equally useless in discovering the smaller winged insects, on which they prey in the dusk of the evening.

But this is improbable.

Can we, however, meditate on the wonderfully rapid turns and evolutions of these creatures in pursuit of their prey, and not allow them the powers of sight to effect the first principle of life, a power not denied to any known animal possessed of a red circulating fluid by the arterial system? To assent to the conclusion which Mr. de Jurine has drawn from his experiments, that the ears of bats are more essential to their discovering objects than their eyes, requires more faith, and less philosophic reasoning, than can be expected of the zootomical philosopher, by whom it might fairly be asked, Since bats see with their ears, do they hear with their eyes? It will not be sufficient for these experimentalists to inform us, that the copious auricles of this class of animals, or their delicate internal structure, are adequate to the double purpose of seeing and hearing, when we perceive, that they are by nature provided with organs of sight similar to what we not only feel most sensibly to be the most inestimable of blessings, but also perceive to be the principal fountains of locomotion in all other animals in the same scale of beings.

Its directing its flight in darkness unaccountable,

Although it cannot be admitted, that the Almighty hand gave to these creatures those most wonderfully constructed organs of sight, without endowing them with visual properties, yet it must be allowed, that there is something extremely astonishing and unaccountable in their unembarrassed flight in total darkness, whether by sealing up their eyes, or by their natural habits of finding their way through all the smaller passages and windings into the inmost recesses of their subterraneous abode. By what occult property they direct their course in total darkness, is perhaps a problem of as difficult solution as that of a swallow returning from the torrid to the frigid zone, to breed in the same nest it had prepared the preceding year, and in which it had performed those functions of nature. Can any human understanding develop the cause, that so unerringly directs the carrier-pigeon to its place of nativity, when previously

but no more than other facts in natural history.

 taken

taken to the distance of five hundred miles? How is the bee instructed to find its hive when captured and taken to a distance? This is inexplicable, and yet no one will dispute the fact. Indeed the practice is common in some countries, in order to find the wild hives; for if two bees are taken near the same spot, and turned out at different points, distant from each other a few hundred yards, if belonging to the same hive, the two lines formed by the direction of their flight will discover the hive to be at the intersection of those lines. These are the mysteries of nature, so impenetrable to the human mind, that we are lost in a labyrinth of wonder at such instinctive endowments, which are incomprehensible to our limited faculties. We have only attentively to examine the operations of nature, and we shall find a thousand instances not less astonishing, than that the bat should find its road without one single ray of light to direct its course*.

Mode of finding hives of wild bees.

Vespertilio Barbastellus.

Gmel. Syst. i. *p.* 48. *Buffon.* viii. *p.* 130. *t.* 19. *f.* 1.
Pennant Quadr. ii. *p.* 561. *Shaw Zool.* i. *p.* 133.
Brit. Miscellany, t. v.

V. barbastellus found in England.

This species has long been known to be an inhabitant of some parts of the European continent, especially France, but, I believe, had not been discovered to inhabit England till the year 1800, when I first noticed it to be indigenous to the south of Devon, and had prepared an account of it for the Linnean Society. Since that period others have occurred in the same county; and we are informed in the *British Miscellany*, that it has been taken in the powder-mill at Dartford in Kent.

The figure and description given in that work are highly satisfactory; but as it is a newly discovered quadruped in

Teats not perceptible.

* Since the preceding account was written, several of both these species of bats have been collected from the same cavern, and in one of the *v. minutus* the abdominal papillæ were more conspicuous than in the former; but not the least vestige of such could be found in the *v. Ferrum-equinum:* it should, however, be remarked, that in these the pectoral teats were equally invisible.

this

this island, and of course little known, it may not be uninteresting to give some additional description of it from specimens in my possession, and to make such further remarks as may conduce to its natural history.

The first I obtained was taken on wing in the village of Milton, which is situate near the coast, and, I believe, was a female. Described.

The colour of this is a dusk-black, intermixed with a few gray-brown hairs towards the rump: the membranes of the wings and tail dusky.

On the 17th of August 1805, I procured a male specimen alive; it was found adhering to a small tree near Kingsbridge.

The length is nearly four inches, of which the tail measures one inch seven eighths; the extent of the wings about eleven inches: weight exactly one hundred grains.

The colour differed a little from that of the former, especially in having the middle of the back and the breast mixed with silver gray hairs; the lower belly, thighs, and behind the vent on the tail membrane more gray. The nose is rounded in front, flat, and cavernous on the top, in which part the nostrils are placed: ears large and black, furnished with a linear valve, and unusually broad at the base, extending forwards, and meeting over the nose, so as to cover the forehead: eyes very small, seated within the membrane of the ear: the teeth numerous in both jaws, and much jagged; in the upper, four cutting teeth, but no canine, and a vacant space between those and the grinders: in the lower jaw six cutting teeth and four canine or longer teeth, and between these last on each side is a small intermediate one; these longer teeth fall into the vacant space in the upper jaw.

Buffon appears to be the first naturalist who recorded this species, and his account of it has been copied by succeeding writers.

It seems to partake of the habits of the common bat; but it may readily be distinguished from *vespertilio murinus*, even on the wing, in the earlier part of the evening, by its superior size, and in being by far the darkest in colour of all the British bats. Upon comparison, the flattened nose, more pointed ears, and particularly the base of Its difference from the common bat.

 these

these coming so forward on the forehead as scarcely to leave any space between, will be found essential characters of distinction.

I have not been able to discover the hybernaculum of this species, but it is reasonable to believe its torpid state is passed in similar situations to those in which all but the *v. ferrum-equinum* and *v. minutus* retire during the colder months; none of which appear to be subterraneous.

VIII.

An Account of the Method of hastening the Maturation of Grapes. By JOHN WILLIAMS *Esq., in a Letter to the Right Hon. Sir* JOSEPH BANKS, *Bart. K. B. P. R. S. &c.**

SIR,

Grapes do not always ripen well in this climate.

IT is a fact well known to gardeners, that *vines*, when exposed in this climate to the open air, although trained to walls with southern aspects, and having every advantage of judicious culture, yet in the ordinary course of our seasons ripen their fruit with difficulty. This remark, however, though true in general, admits of some exceptions, for I have occasionally seen trees of the common *white muscadine*, and *black cluster grapes*, that have matured their fruit very well, and earlier by a fortnight or three weeks, than others of the same kinds, and apparently possessing similar advantages of soil and aspect.

Earliest on old trees with long trunks,

The *vines* that ripened the fruit thus early, I have generally remarked, were old trees having trunks eight or ten feet high, before their bearing branches commenced. It occurred to me, that this disposition to ripen early might be occasioned by the dryness and rigidity of the vessels of the old trunk obstructing the circulation of that portion of the sap, which is supposed to descend from the leaf. And to prove whether or not my conjectures were correct, I made incisions through the bark on the trunks of several vines growing in my garden, removing a circle of bark from

from the circulation being obstructed.

Incisions through the bark, leaving the alburnum naked,

* Horticultural Society, vol. I. p. 107.

each

each, and thus leaving the naked alburnum above an inch in width completely exposed; this was done in the months of *June* and *July*. The following autumn the fruit growing on these trees came to great perfection, having ripened from a fortnight to three weeks earlier than usual: but in the succeeding spring, the vines did not shoot with their accustomed vigour, and I found that I had injured them by exposing the alburnum unnecessarily.

occasioned the fruit to ripen early.

Last summer these experiments were repeated; at the end of *July* and beginning of *August*, I took annular excisions of bark from the trunks of several of my vines, and that the exposed alburnum might be again covered with new bark by the end of autumn, the removed circles were made rather less than a quarter of an inch in width. Two vines of the *white Frontiniac*, in similiar states of growth, being trained near to each other on a south wall, were selected for trial; one of these was experimented on (if I may use the term), the other was left in its natural state, to form a standard of comparison. When the circle of bark had been removed about a fortnight, the berries on the experimented tree began evidently to swell faster than those on the other, and by the beginning of *September* showed indications of approaching ripeness, while the fruit of the unexperimented tree continued green and small. In the beginning of *October* the fruit on the tree that had the bark removed from it, was quite ripe, the other only just began to show a disposition to ripen, for the bunches were shortly afterwards destroyed by the autumnal frosts. In every case in which circles of bark were removed, I invariably found that the fruit not only ripened earlier, but the berries were considerably larger than usual, and more highly flavoured.

The experiment repeated,

the fruit ripened earlier, and improved in size and flavour.

The effects thus produced I can account for only by adopting Mr. Knight's theory of the downward circulation of the sap, the truth of which these experiments, in my opinion, tend strongly to confirm. I therefore imagine by cutting through the cortex and liber without wounding the alburnum, that the descent of that portion of the sap which has undergone preparation in the leaf is obstructed and confined in the branches situate above the incision; consequently

Theory of the process.

sequently the fruit is better nourished and its maturation hastened. It is certainly a considerable point gained in the culture of the vine, to be able to bring the fruit to perfection, by a process so simple, and so easily performed. But lest there should be any misconception in the foregoing statement, I will briefly describe the exact method to be followed by any person, who may be desirous of trying this mode of ripening grapes. The best time for performing the operation on vines growing in the open air is towards the end of *July*, or beginning of *August*; and it is a material point, not to let the removed circle of bark be too wide: from one to two eighths of an inch will be a space of sufficient width; the exposed alburnum will then be covered again with new bark before the following winter, so that there will be no danger of injuring the future health of the tree.

Proper time of performing the operation.

It is not of much consequence in what part of the tree the incision is made, but in case the trunk is very large, I should then recommend, that the circles be made in the smaller branches.

Caution.

It is to be observed, that all shoots which come out from the root of the vine, or from the front of the trunk situate *below* the incision, must be removed as often as they appear, unless bearing wood is particularly wanted to fill up the lower part of the wall, in which case one or two shoots may be left.

Applicable to vines in forcing houses,

Vines growing in forcing houses are equally improved in point of size and flavour, as well as made to ripen earlier by taking away circles of bark: the time for doing this is when the fruit is set, and the berries are about the size of small shot. The removed circles may here be made wider than on vines growing in the open air, as the bark is sooner renewed in forcing houses, owing to the warmth and moisture in those places. Half an inch will not be too great a width to take off in a circle from a vigorous growing vine, but I do not recommend the operation to be performed at all in weak trees.

and perhaps other fruits, particularly figs.

I think that this practice may be extended to other fruits, so as to hasten their maturity, especially *figs*, in which there is a most abundant flow of returning sap; and it demonstrates

strates to us, why old trees are more disposed to bear fruit than young ones. Miller informs us, that the vineyards in *Italy* are thought to improve every year by age, till they are 50 years old. It therefore appears to me, that nature, in the course of time, produces effects similar to what I have above recommended to be done by art. For, as trees become old, the returning vessels do not convey the sap into the roots, with the same facility they did when young: thus by occasionally removing circles of bark, we only anticipate the process of nature*; in both cases a stagnation of the true sap is obtained in the fruiting branches, and the redundant nutriment then passes into the fruit.

No portion of the inner bark must be suffered to remain.

I have sometimes found, that, after the circle of bark has been removed, *a small portion of the inner bark has adhered to the alburnum*: it is of the utmost importance to remove this, though ever so small, otherwise in a very short space of time the communication is again established with the root, and little or no effect produced. Therefore in about ten days after the first operation has been performed, I generally look at the part from whence the bark was removed, and separate any small portion, which may have escaped the knife the first time.

I am, Sir,

Your obedient humble servant,

JOHN WILLIAMS.

Pitmaston, Worcestershire,
20th *April*, 1808.

* Hence we may infer, that trees thus treated will have their decay accelerated, and their natural duration shortened. C.

IX.

IX.

An Essay on Manures. By ARTHUR YOUNG, *Esq. F.R.S.**

Arrangement of the subject.

MR. Young first arranges the treatment of his subject in the following order. 1. The nature of the manure. 2. Its properties. 3. Collecting. 4. Preparation. 5. State in which applied. 6. Application. 7. Season when applied. 8. Quantity. 9. On what soil.

He next classes manures in two divisions. 1. Such as are made or dug on a farm. 2. Such as are usually purchased. The latter he subdivides into animal, vegetable, and fossil. In the first division comes

1. *Marle.*

Marle.

The marles most common in England are clay, stone, and shell marle. Some distinguish them by their colours, as white, red, blue, black, &c.; but the colour deserves no attention except as indicative of iron.

Its nature.

They are usually composed of sand, clay, and calcareous earth. The red and black have a small quantity of iron. A marle from Cheshire had 1·7 per cent. Even in the whitest prussiate of potash will almost always detect some iron. The calcareous earth varies from 25 to 80 per cent. One of the best clay marles contained 40 calcareous earth, 50 clay, 8 or 10 sand, and clear signs of some iron. It falls in pure water, and by exposure to the air. The clay contains generally a small portion of iron, a little volatile alkali, and some sulphuric acid; and even when deprived of

* Abridged from the Bath Society's Papers, vol. X, p. 97. This essay was written in consequence of the following subject being announced for a prize, which it obtained. "The Bedfordean gold medal will be presented to the author, who, at or before the first meeting in November 1804, shall produce to the Society the best essay, founded on practical experience, on the nature and properties of manures, and the mode of preparing and applying them to various soils: in which essay shall be pointed out the cheapest manner of collecting and preparing the different kinds of manures, and the state, season, and quantity, in which they should be applied."

all

all organic matter yields hidrogen gas. Phosphorus may be gained from all calcareous earths.

What renders it particularly valuable is the calcareous earth it contains. But we do not yet know what ought to be the quantity of calcareous earth in a soil. The best specimen analyzed by Giobert had 6 per cent; by Bergman, 30; by Dr. Fordyce, 2; and a rich soil quoted by Mr. Davy had 11. This is an inquiry, concerning which the author has made many experiments, and on soils of the most extraordinary fertility. In one he found 9 per cent; in another 20; in another 3: and in a specimen of famous land, procured from Flanders, 17. Many poor soils however possess nearly the same proportion as the most fertile: and on comparing every circumstance he is disposed to conclude, that the necessity of a large proportion of calcareous earth depends on the deficiency of that organic matter, which is convertible into hidrogen gas. If the farmer find by experiment, that his soil contains but a small quantity of organic matter; or know by his practice, that it is poor, and not worth more than 10, 15, or 20*s.* an acre; it ought to have 20 per cent of calcareous earth in it. If on the contrary it abound with organic matter, and be worth in practice a much larger rent, it will not require marling, though it contains but 5 per cent of calcareous matter, or even less. Marles likewise give tenacity and firmness to a soil, and for this the clay marles are to be preferred. Some soils abound with acid particles, which are prejudicial; and these are neutralized by the calcareous earth. Properties.

The earth found in vegetables is for the greater part calcareous. Hence we may presume, that this earth should make a part of the soil. Lord Dundonald calculates, that all the calcareous earth to be obtained from the vegetable produce of an acre of most crops will not exceed eighty pounds: but if even this quantity be required for every crop, the necessity of occasional supply appears.

Marle is generally obtained by digging, but it is also dredged up from the beds of some rivers. White shell marle, and a very light white species, are found under bogs, and at the bottom of lakes. No person, whose land wants marle, where it is not generally known to exist, should be satisfied Collecting.

satisfied without the most careful examination by boreing. A borer for twenty feet depth does not cost above £3, for 80 feet not above £21, and is used without difficulty by any common workman.

Application. Marle requires no preparation. It is best applied on leys: and the longer it lies on them before it is ploughed in, the better. It should not be ploughed in too deep. The best way therefore is, to plough the ley shallow for pease. To turnips there is but one objection, the giving so much tillage so early after the improvement. Potatoes are mischievous for the first crop after land has been marled. Next to leys, fallows are the best to receive marle. When the farmer has a choice, on wet and heavy soils it should be summer work, and on dry ones it may be winter.

Season.

Quantity. The quantity employed is of great importance. From 120 to 150 cubical yards per acre being laid on a poor sand, the productiveness of the land has been injured for twenty years. Half this quantity would have done good. It is better to marle twice, than apply too much at once. On poor, loose, wet loams more may be used than on loose sands. On loose peat bogs, and on moors, the greater the quantity the greater the improvement. Where the object is to give calcareous earth, the quantity should be small, as from ten to twenty tuns.

Soil requiring marle. The defect of a soil must be understood, before a wise farmer will put himself to the expense of marling. Every day's experience will inform him, whether his land want tenacity and consolidation; but the want of an addition of calcareous earth as a food of plants can be discovered only by analysis. Other circumstances deserve attention. If the chrysanthemum segetum, corn marigold, rumex acetosella, sheep's sorrel, or polygonum pennsylvanicum, abound, the experienced farmer will pronounce, that the land wants marling. Turnips producing deformed strings of roots, without swelling into the proper globular form; or being subject to the well known distemper of the anbury; both afford a proof of too much looseness of texture, and suggest consolidation by clay marle, after which these evils vanish. The erica vulgaris, common heath, or ling, is generally a proof of an acid soil; and all peat soils are found

on

on analysis to contain a considerable quantity of the gallic acid. Some have been rendered quite sterile by acids. A stratum of moss in Scotland was so impregnated with vitriolic acid, that from four pounds of it one pound of green vitriol was extracted. In a bog in Bedfordshire sulphate of iron abounded in almost equally extraordinary degree; yet it has been converted into one of the finest water meadows in England by his grace the late Duke of Bedford. Wherever such soils are found, marle is sure to have great effect from its calcareous earth. For wet but loose loams, which when manured are more productive of straw than corn, clay marle is a cure, and attended with unquestionable profit. Another quality of these loams is that of being uncommonly pestered with the red worm; and it is a singular quality of marle, to lessen this evil considerably. Whatever gives them a firmer texture has a tendency to this effect.

2. *Chalk.*

Chalk.

Properties.

Chalk in its properties nearly resembles marle, but it contains a much larger proportion of calcareous earth. It renders tenacious clay more dry and friable, which stone marle alone will not. It is also more common to chalk grass lands than to marle them; and it works a capital improvement on low, coarse, sour meadows, rendering them firmer, and improving the sweetness of the herbage.

Method of procuring it.

It is commonly dug from pits like marle: but the general practice of Hertfordshire is to sink shafts for it. The chalk-drawers travel in gangs; chamber the shaft all round, leaving columns to support the incumbent earth; and draw up the chalk in buckets. They will wheel it on to the land for 8*d.* the load of twenty-four bushels to the distance of twenty poles from the shaft.

Quantity used.

It is generally used in much smaller quantities than marle. In Essex, whither it is brought by sea from the Kentish coast, from five to eight waggon loads per acre are attended with more remarkable effect than even dung itself, if the land have not been chalked before. More than forty cubical yards are seldom spread on an acre.

Effects.

The most remarkable effects attending it appear to be upon

upon good sound loams, worth from 15*s.* to 20*s.* an acre. Six or seven waggon loads per acre are seen immediately in the crops, and to an inch. Chalk presently gives the land a reddish colour, so that the part of a fallow which has been chalked will be discernible at a distance from this tinge. A singular circumstance observed in Essex is its being an enemy to what their farmers call grazing, or running to turf. A field, which before chalking will run of itself to a fine head of white clover, does so no longer after chalking. The chalk used there is not soft, but rather hard. The sharpest frosts leave many lumps unbroken, which must be done with pickaxes; and the hard bits, which break to a clear white, are better than those that crumble between the fingers. This is to be attributed to the nature of the soil, which is rather too stiff for turnips.

Where applicable.

Soils abounding spontaneously with sorrel are highly improvable by chalk. It is used successfully on all soils, on which marle is found to answer. It is not a general favourite in Norfolk for poor sands, or even middling ones; but some farmers of considerable note for accuracy of observation have of late used very hard chalk, and with great success. On all moors, peat bogs, and peat fens, every species of calcareous earth may be applied with singularly good effect; and as chalk abounds more than marle in this earth, it is full as valuable on them, if not more so.

3. *Lime.*

Lime.

Every kind of calcareous stone, being in fact a carbonate of lime, may be converted into lime by expelling its carbonic acid and water by means of fire. In this state it is caustic, and has a strong power of reabsorbing moisture, and likewise carbonic acid, if exposed to the atmosphere. As limestones generally contain a portion of clay and sand, these will remain mixed with the calcareous earth in the lime. This is of little consequence, only diminishing the quantity of calcareous earth. But sometimes they have a mixture of magnesia, and this has been said to be detrimental to vegetation. Limestone that contains magnesia is generally of a brownish hue, or fawn colour; but none is found in a stone that breaks blue.

As

As lime after some months exposure is converted into chalk, it must have similar effects with regard to supplying calcareous earth; but it will not give tenacity to sand like marle, or friability to clay like chalk. When laid on in its caustic state, it destroys the spontaneous growth of soils: and this is a very valuable quality, where this growth is a nuisance. The truth of this observation is visible on limed moors. Its properties.

The most material distinction in the application of lime is that of spreading it fresh in its most caustic state, or keeping it till it is slacked, and has reabsorbed more or less carbonic acid. Application.

On all soils in a state of nature, and greatly abounding with undecayed vegetables, which are required to be speedily destroyed, it should be spread hot from the kiln, as it is termed; that is, in its most caustic state. In other cases it is slacked, before it is spread. Upon waste lands the causticity has an evident and necessary effect; but not on cultivated lands, which this quality of the substance while deprived of its carbonic acid would tend to prejudice rather than improve.

A truly practical husbandman of great experience, Mr. Craike, of Arbigland, gives directions for the application, which merit attention. "Let the whole quantity of lime, intended to be used on any given field of moderate size, be laid in one heap, where water can be had most conveniently. Let it be there thoroughly slacked; and immediately after it is cold, which it will be in a day or two, fill the carts, and spread the lime out of them with shovels equally over the surface. The more common method of laying it down in small heaps over the whole field, to slack by rain, is very erroneous. It is liable to get too much rain, which, in place of reducing it into a fine powder, converts it into a running mortar, in which state it will neither spread equally nor mix with the soil*." And for the same reason, Mr. Wight remarks, both the ground and the lime should be quite dry at the time of spreading. In Dumfriesshire, quick lime being compared with some that had lain in a heap for

* Trans. of the Dumf. Soc. No. II, p. 34.

several

several years in consequence of a lawsuit, the latter did much more good than the former.

Season.

Where improvements are carrying on upon a large scale, and draw-kilns are kept at work throughout the year, the choice of season becomes of secondary importance: in other cases liming should no more go on in winter than building. It may be continued from March to October, but summer is the best season. It should be spread on a ley one full year before ploughing, that it may have time to fix itself firmly in the sward. If ploughed too soon it falls to the bottom of the furrow, and will be the sooner lost, for it continually sinks. Three years before breaking up a ley, part was limed with three hundred bushels an acre; the remainder was limed with an equal proportion only one year before it was broken up. The former produced oats 10 for 1 of the seed, the latter 6 for 1.

Quantity.

In common cases the quantity ought to be guided by a chemical analysis of the soil. The largest quantities have been spread, and with propriety, on bogs and peat moors, and on mountains. The Bishop of Landaff speaks of a thousand bushels an acre on moors in Derbyshire applied with great success. Five or six hundred are not uncommon there. Lord Chief Baron Foster, in Ireland, went as far as to three hundred barrels, on a moory waste; and found, that the greater the quantity the greater was the improvement. Dr. Anderson tried from one to seven hundred bushels an acre, and found the good effect to increase regularly with the quantity. In more common cases the quantities vary in general practice from thirty-six to a hundred and sixty bushels.

Where applicable.

On peat bogs, peat moors, and mountains, the utility of lime cannot be questioned. Experiments on every scale, and under a very great variety of circumstances, speak a uniform language: the benefit of applying lime is great and decided. On liming Kedgley moor, in Northumberland, covered with ling, the ling was killed, and three tuns an acre of white clover were mown without sowing any. Part of Meriden heath, in Warwickshire, was fallowed for a year, ten acres trebly folded with a thousand sheep, ten acres

acres well dressed with good rotten dung, ten acres well limed, and the whole sown with oats and seeds. The part folded had not a bag of oats an acre, and the seeds were not worth saving: that which was dunged succeeded very little better: while that which was limed produced a very excellent crop of oats and seeds.

In Glendale ward, Northumberland, the soil is naturally dry, duffy, light, full of fibrous roots, and, when in fallow, on passing over it you sink to the ankles. After it is sufficiently limed, the fibrous roots disappear, the soil becomes denser, firm to tread, retentive of moisture, and produces better and more abundant crops of grain: and, if laid to grass, white clover appears to an inch where the lime was spread. Even on a burning sand four chaldrons an acre have had a striking effect; but then the sand was covered with a mossy sward.

Lime does worst on a cold hungry clay. It cannot succeed, where in the farmer's language it has nothing to work upon; where water deprives it of its most material properties; or where frequent repetitons have given a full dose of calcareous earth, and consumed every vegetable particle. After paring and burning lime is at best useless, the vegetable fibres being already destroyed by fire.

Limestone, & limestone gravel.

Where calcareous manures are required, powdered limestone may be employed with excellent effect. Perhaps it may be questioned, whether limestone gravel be not the best of all manures for improving a peat bog.

4. *Clay, Loam, and Sand.*

Clay, loam, & sand.

The effect of these depends on the deficiency of the soil. Clay is every where beneficial on sand: but sand is not equally so on clay, for many clays contain far more sand in their composition, than farmers are apt to suspect. Sandy loams are frequently considered as clays, because they are heavy for want of effectual draining.

Sea sand.

Sea-sand partakes of another class of manures. It contains muriate of soda; and if it be a shelly sand it is so far allied to shell marle.

5. *Burnt Clay, Marle, and Earth.*

Burnt clay and earth.

In various parts of the United Kingdom it has been a practice to burn clay, and clay marles, in large heaps, and to spread the ashes as manure. The nature and properties of burnt earth must vary with the portion of it which is calcareous, as this is converted into lime by calcination. Burning clay breaks its cohesion entirely, and reduces it to a permanent state of friability, which does not permit it to combine with any other substance: the sulphuric acid, which most clays contain, is dissipated: the iron and the clay itself are oxigenated: and a faculty of generating nitre is given in some cases. In its burnt state also it has a power of combining with the salt of urine. Burnt clays, says Dr. Darwin, when strewed on the ground, may contribute to vegetation, by their parting with their oxigen in a fluid, not a gaseous form; which, united with carbon, or phosphorus, or nitrogen, might supply nutritious fluids to the roots of vegetables. Its texture is extremely beneficial in dividing and attenuating the harshness of stiff soils, and rendering them more absorbent. These circumstances are amply sufficient, to account for the benefit which many persons have derived from the practice of burning clay and marles. Mr. Leslie, in Ireland, made great exertions in this way: Mr. White Parsons, in Somersetshire, has burned the earth out of ditches and drains successfully: and Mr. Boys, in Kent, has been long in the habit of doing it, paying his men sixpence per load of ashes for digging and burning.

(To be continued in our next.)

X.

X.

On the Construction of Theatres. In a Letter from RICHARD LOVELL EDGEWORTH *Esq., F. R. S. and M. R. I. A.*

To Mr. NICHOLSON.

SIR, *Edgeworthstown, March* 6, 1809.

THE public, by the loss of two theatres in one winter, must be anxious about the plans on which those edifices are to be rebuilt: they will not be satisfied with the opinion of a single architect, they will require an open discussion of the principles, and plans upon which a new theatre is to be constructed; this they have a just right to demand, for their lives and properties are at stake. Every family in London might have mourned the loss of some relative, had the play-houses been filled at the time of the accident; and the whole city might have been burned to ashes by either of the conflagrations.

The building of a theatre an object of public concern.

We are to consider not only the loss of lives by the immediate disaster, but also the apprehensions, which the audience must feel for some time to come; and the anxiety, which those who remain at home must suffer during the absence of their friends at the theatre. Nothing should be left to embitter the cup of innocent pleasure, and "assurance should be made doubly sure," where great hazards are run, from no greater motive than the hope of an hour's amusement.

It cannot be too secure, as it should prevent even apprehension.

Covent-garden playhouse is now rebuilding without any previous appeal to the public, that I have heard of, as to the plan or precautions, that are to be followed in its construction. I know, that some hints were sent on these subjects, which were not even considered, at least not noticed, till after the plan was arranged. Surely it must be infinitely more advantageous to the proprietors and to the nation, that a short delay should take place before a plan is ultimately arranged, than that a new theatre should be opened ten days sooner, or ten days later.

The public should be called on for hints.

The glaring defect, or to speak more properly, the obvious blunder in the building of Drury-lane theatre, was the

Timber should not be intro-

duced as a frame-work.

introduction of timber as a frame work for bricks and stone; this is a fault common to buildings in London, where the public safety is without hesitation sacrificed to the interests of individuals.—But to construct a wooden theatre is an absurdity too gross, to pass without animadversion. A frame-work of timber, filled with cores of brick or stone, and cased perhaps with brick or plaster, is opened for the reception of the public, who are to run the risk of sudden destruction from a spark of fire, or a snuff of candle, from the fireworks and lightning of comedy and tragedy, of pantomime and farce, without any probable means of escape, or any security, except what a few hogsheads of water in a cistern on the top of the house can afford.—No future prologue at the opening of a new theatre could re-assure the audience upon this subject.

Time should be allowed for obtaining information from every quarter.

From a view of these considerations I hope it will appear incumbent upon those, who rebuild Drury-lane, to take time for receiving information from every quarter whence it may be expected: instead of hurrying forward to a beginning before they have well considered the end. A remarkable observation made by that great engineer Mr. Smeaton, in his account of the building of the Eddystone lighthouse, should never be forgotten by those who direct, or by those who undertake extensive public works.—" No resolution of " the proprietors," says he, " ever conduced more to ultimate success, than their leaving me at liberty *(as to time)*; " had they been of the same temper and disposition as by " far the greatest part of those who have employed me, " both before and since, their language would have been, " *Get on, get on, for God's sake get on,* the public is in " expectation, get us something speedily, *to show,* that we " may gain credit with the public."

Observations of Mr. Smeaton.

Architects should be engineers.

Architects and engineers are so nearly connected with each other in the objects of their pursuits, that it would be well both for them and for the public, if every architect were an engineer, and every engineer an architect. That this is not always the case, we have melancholy instances to prove.

Society of civil engineers in London.

There is a society of civil engineers in London, of which Sir Joseph Banks is president, consisting of men of undisputed

puted talents and information. Would it not be advisable, to consult this board? No harm could possibly arise from such application, and much good might be the consequence. If in the multitude of counsellors there may be some delay, there is probably much safety. should be consulted.

Having now animadverted upon the steps that should be taken, before any plan is ultimately settled, I shall venture to offer a few hints upon the construction of a theatre. If any thing, which I throw out, should become an object of discussion, I trust that I may have an opportunity of explaining what I propose; and if any thing be adopted from my suggestions, that it may not be followed, without my being acquainted with the mode of execution. Many new attempts fail of their object by the introduction of additional ideas, that appear plausible; or by the omission of small circumstances, that seem in the original plan to be of no material consequence. Plans should be executed under the eye of the proposer, or followed strictly.

In building a theatre, Leading objects in building theatres.

1. Security to the audience is the first and most necessary object.

2. Facility of ingress and egress.

3. Facility of seeing and hearing.

4. Convenience to the performers.

5. Space for scenes, with proper openings for the machinery.

6. And lastly, expense.

1. *To ensure safety*, common sense points out, that as little timber and as small a portion as possible of combustible materials should be employed. The outside walls should be constructed of stone—the coins of large blocks of stone closely jointed, depending upon their own bearings and not made apparently compact by mortar. Bricks for the internal structure should be made under proper inspection, and not worked hastily up, to fulfil a contract. All the joists, rafters, and principals, and the framework of the partitions, should be iron. The framework of the roof should be of the same metal, with a covering of copper. No plumber should be permitted to exercise his dangerous For safety avoid timber, and substitute iron. Roof with copper, and admit no plumber's work.

gerous trade in the construction of any part of the building.

Iron not expensive if employed with skill.

It may at first sight appear, that the substitution of iron for timber must be enormously expensive—and it would be enormous, if scientific care were not taken, to calculate the stress and strength of every part of the structure where iron was to be used, and to frame the material together upon mechanical principles of strength and lightness.

Roof of iron cheaper than timber.

Hollow brick flooring.

As to the roof, it could no doubt be made lighter and cheaper of iron than of timber at the present price of that material. Cotton mills are frequently floored with hollow bricks, which are light; and these may be covered with carpetting.

Wood that does not flame advisable.

Deal may be prepared so as to be less inflammable.

Many other parts of the theatre might be constructed of iron and copper; and stucco might be introduced in many places instead of wood. There are kinds of timber that do not flame; these, though not very durable, might be employed for floors and benches. And where deal is absolutely necessary, it may be covered or imbued with a wash, that in some degree will retard inflammation. After the wood work that requires painting has received two coats of oil paint, it may be finished with a coat in distemper, which may frequently be renewed at small expense, and without the disagreeable smell of oil paint.

The private apartments should be heated by steam; and the boiler should be adapted to work an engine for supplying and throwing water.

To heat the green room, dressing rooms, and the withdrawing rooms, steam might be advantageously employed; and the boiler to supply the steam should be so placed, as to serve at a moment's warning, to work a steam engine of force sufficient to draw water at once from the Thames, and to drive it with a strong impulse wherever it should be wanted. This steam engine should be strongly enclosed in a building, to which access on every side could be easily obtained.

Avenues.

2. Some of the theatres at Paris have commodious avenues; but not one theatre in London has been so placed, or so constructed, as to afford tolerable convenience either to the higher or lower class of spectators.

Private property intervenes so much, that it is scarcely to be expected, that any great improvement can be made in

in this respect, by enlarging the area round the site of the late building.

Whether a more convenient situation might be selected, I do not pretend to know; but a theatre built on the old foundation might be rendered extremely commodious as to its entrances, or *vomitories*, as the ancients called the avenues to their amphitheatres. The entrances might be made very commodious

If the whole building were raised upon arches of a height sufficient to admit carriages, and if numerous flights of stairs were constructed within the piers which support these arches, the audience might depart commodiously in different directions, without confusion or delay. by raising the building on arches.

The colonnades formed by pillars properly disposed would permit alternate rows of carriages. Company might descend from the boxes almost immediately into their carriages: passages for those who were on foot might be railed off, and rendered secure.

This plan would be attended with considerable expense; but it might be counterbalanced by sparing one of the higher galleries, which lately injured the *audibility* of the performance, without adding much to the profits of the house. Besides it might be so managed, that tickets for the admission of carriages under the *piazzas* should be issued, which would cover the expense of their construction. This expense might be compensated.

3. *Facility of seeing and hearing.*—As to seeing I believe that very little can be said, but what is obvious to every person of common sense; the actors and the spectators have in this respect opposite interests. It is the interest of the actors, to have that part of the house, which contains the audience, as large as possible. On the contrary it must be the wish of the audience, within certain bounds, to be near the stage; and in all cases, the audience must wish, that every part of the pit, galleries, and boxes, should be equally commodious for seing. Now in a large theatre this is impossible. To extend the pit and boxes, they must recede from the front of the stage; they cannot be extended in breadth without shutting out the view from the side boxes. Facility of seeing,

Little

and of hearing. Little inconvenience was felt as to seeing at Drury-lane; but every body, who wished to hear, complained. As to the actors, to make any impression, they were obliged to raise their voices above the natural pitch; to substitute pantomimic gesticulation, in the place of inflexions of voice; and to use contortions of features instead of the natural expression of the eyes, and the easy movement of the countenance. It is in vain, that critics inveigh against the bad taste of those, who prefer show, and pantomime, and processions, and dancing, and all that the French call *spectacle:* unless we can hear the sentiments and dialogue, it is useless to write good plays; but all the world loves *spectacle.* Both these tastes should be gratified. Garrick, as I have heard him declare, was always entertained with a pantomime: he told me how many times he had seen Harlequin Fortunatus with delight—the number I forget, however I am sure, that it far exceeded the number of times any man could hear a good comedy or tragedy. Surely the literary and the visual entertainment of different spectators might be gratified. In the first place, the audience-part of the theatre ſhould be left smaller, and lower, than it was at Drury-lane. Its shape might undoubtedly be improved, by constructing it according to the known laws of accoustics: but this, if rigorously attended to, would contract the space so, as to affect too much the *receipts* of the house.

Garrick fond of pantomime.

Audience part.

Stage and scenery.

The area for the stage might be as large as it was formerly; but the scenery should be adjusted so as to contract the stage to reasonable dimensions. To confine the voice, the wings should have leaves, or flaps, hinged to them, so as occasionally to close the space between the wings, leaving sufficient room for exits and entrances. When large objects require admission, these leaves might be turned back, and would then allow the same space as usual between the wings. This would be an additional convenience to the actors, while they stand in waiting to enter on the stage, as it would screen them from the cold. The ceiling of the stage, which at present is made by strips of painted linen hanging perpendicularly, should be made of well varnished iron or copper

Ceiling of the stage.

copper frames, turning upon centres so as to open at pleasure like venetian window-blinds; and by this means to contract, at will, the opening of the ceiling, and to conduct the voice of the performers towards the audience. The current of air, so as it does not amount to wind, should flow from the stage to the audience. By experiments tried upon sound by Sir Thomas Morland and some other members of the Royal Society, it appeared, that the propagation of sound was prodigiously obstructed by the assistance or opposition of a slight current of air. We are told by Vitruvius, and Lipsius, that the sound of the actor's voice was increased in a surprising manner by brazen vessels placed under the seats of the audience.

A gentle current of air should flow from the stage.

Sound increased by brazen vessels under the seats.

No satisfactory account remains of the manner in which this desirable effect was produced. It would not however be difficult to try experiments on this subject in any one of our theatres when it is vacant.

About 40 years ago I happened to go with a friend into a large cockpit at an inn at Towcester. My friend, who was at the opposite side of the pit, appeared to me to speak with a voice uncommonly loud and sonorous. Upon my inquiring why he spoke in that manner, he said, that he had not raised his voice above its ordinary pitch. Upon looking about I perceived a large earthen jar behind me, which proved the cause of this increase of sound: for upon repeated trials the voice of my friend sounded as usual when I stood in any other part of the cockpit, but that in which the vase was placed. To the best of my recollection the jar was about five feet high, and twenty inches in diameter. I remember well, that it rung clearly, but slowly, when struck with the knuckle. By what means, and by what materials, the pulses of sound may be best returned for the purposes we have in view, is a subject for the joint efforts of mathematics and experiment.

Sound increased by an earthen jar behind the auditor.

Among other expedients pannelling the backs of the boxes with thin elastic plates of brass might be tried.

Expedient suggested.

A saving and advantage would certainly arise in all cases from using iron, or copper, instead of wood; they would not require renewal for many years, and they would be a preservative against

against fire. The prompter's box might certainly be improved, so as to throw the prompter's voice more distinctly upon the stage, and to prevent its being heard by the audience.

Comforts of the performers should be studied.

4. *Convenience to performers.* Notwithstanding the reveries of Rousseau, and the declamations of the overrighteous, actors have risen in the estimation of the public. We have seen with rational and sincere pleasure the excellent conduct of many female performers. I consider this reform as highly advantageous to morality, and it becomes a duty in the managers of a theatre, to accommodate the performers with every possible convenience, so that they may enjoy that English word *comfort*, which in all situations of life tends to promote independance and morality.

Speaking pipes.

It is scarcely necessary to add, that pipes to speak through should be laid from the green room to every apartment of the actors.

Expense.

6. I have left the article of *expense* to the last, because whatever essentially tends to the convenience and gratification of the public will always find sufficient supplies from the liberality of Britain. A small addition to the price of tickets would amply defray the expense, that would be incurred by any real improvements.

If the united efforts of men of science and men of practice were directed to this object, we might expect to see a theatre superior to any on the continent, adapted both to the purposes of splendid exhibition and of true comedy; where our children might be entertained with the "Forty Thieves," and ourselves with "The Rivals" and "The School for Scandal."

R. L. E.

XI.

XI.

Plan for Preventing or Suppressing Fires. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

THE destructive fires, that have recently taken place in London, have induced me to compress a few ideas on the subject of watching public buildings, which have arisen from a desire to form a plan of safety for a building in which I am myself interested. I shall confine these observations to the prevention or suppression of fire, in such a theatre as that lately in Drury Lane, or Covent Garden; and, if they are calculated for a place in your valuable Journal, they are at your service.

Watchmen to prevent fires.

Let it be supposed, that such a building is directed to be nominally divided into convenient sections, each capable of being and actually attended to by one watchman. A small chamber, or any other space, in addition to and distinct from these, in a proper situation, shall be occupied by a person to direct or check these watchmen. The direction may be exercised ordinarily without leaving this chamber, in the following manner. Let there be one clock for each watchman, of a certain construction (which is at present partially in use, and proved to accomplish purposes similar to the object of the present paper) fixed in the chamber of the director of the watchmen; each clock communicating with the section of its proper watchman by cranks and wires, or otherwise, in such a manner, that by pulling the wire he shall be able to effect a visible alteration on the clock at a precise moment, as agreed upon, conformably with the construction of the clock, but not at any other moment. This clock shows the usual division of time, and has also a revolving frame in which pins are placed in sockets capable of being pressed down at particular times only, as above stated. Thus, by the use of this clock, a watchman's vigilance or neglect may be proved by the evidence of the clock itself.

Method of ensuring their vigilance.

Suppose,

Its operation.

Suppose, for example, this clock be so constructed, that a pin shall be pressed down every quarter of an hour, and the proof of this being done shall rest with the director of the watchmen, in the first instance, simply by looking at the clock every quarter of an hour; it is evident, that the neglect of a watchman cannot exist longer than this space of time, if the director fail not in his duty. This man should himself be watched with the most scrupulous suspicion, and detected in his own failure, in the same manner as he should detect the failure of the watchmen; that is, by the proof of a clock on the same principle as the above, placed on the outside of the building, and under the absolute examination of the police, or any other superintendance satisfactory to those most interested. If the director be correct, instant alarm would be led to the section of any watchman whose duty should appear to be neglected; and if the director be incorrect, the alarm would be ulterior, and as active as in the case of positive danger. It may be thought difficult for one man to examine many clocks at the same moment: if it should be found so each clock might be set differently, and every watchman have a clock in his section set by his proper clock in the director's chamber.

Thus speedy assistance ensured.

Hence, in case of fire, a discovery would not only soon take place, but personal assistance would be on the spot, and, with proper access to water at all times ensured, with the best means of applying it, an increase of the first evil would almost certainly be prevented, until additional assistance could be procured: and alarm bells or other signals, by the sound or character of which the particular building might be made known to firemen, could, if necessary, be instantly sounded or displayed, and a constant influx of proper persons would take place in the very infancy of danger.

The plan has been tried.

It is not improbable, that this plan may be thought by many persons too elaborate and expensive. To such it will be satisfactory to know, that very extensive and valuable buildings in my neighbourhood, the property of some highly ingenious and respectable gentlemen (one of whom is the inventor of the clock) have been watched for several

years

years by a single watchman, checked by this clock alone, and with extremely few evidences of neglect. This is the result of fines, &c., begun with judgment, and enforced with strictness. But one objection can be offered against this, namely, that the morning only brings the proof of the watchman's conduct, when nothing can be opposed to his neglect but fine or dismissal; while the hours of greater danger must be left to his discretion, and the fear of punishment. Objection.

As many modifications of plans like this are easily devised, and new arrangements made in application to practice, not readily imagined before, it is deemed unnecessary to enter into detail, or to attach any specific regulations for each department, or for the ultimate execution of the whole. If it is satisfactorily made out, that the plan is practicable and useful, a slight calculation will show the expense to be insignificant, when compared with the object, or even with the premium of insurance. This plan may be adapted to circumstances. The expense not an object.

I am, Sir,

Your obedient servant,

Derby, May 11, 1809. M. K.

Annotation. Respecting register clocks for the useful purpose indicated by M. K. see our Journal, vol. V, p. 133.

XII.

On the Method of taking Transit Observations. In a Letter from a Correspondent.

To Mr. NICHOLSON,

SIR,

IN the second volume of your Journal, Mr. Ezekiel Walker, after mentioning Dr. Bradley's method of taking transit observations, by noting the proportional distance of the Method of taking transit observations.

the star from the wire at the two beats of the clock, proposes another, which he thinks superior. This consists in noting the time when the centre of the star comes to one side of the wire; which, he observes, is a real line, and not as in Bradley's, a line drawn by the strength of imagination down the middle of the wire, parallel to the sides. I have tried both these methods with nearly the same success, and must confess that, after all, I am very much at a loss to conjecture, how the fractional part of a second can be estimated in either of these ways to that nicety it appears to be done. In the observations made at Greenwich I observe, the time of a star's passing the meridian is always expressed to the hundredth part of a second. How this extreme precision is obtained, as I am at a loss to conjecture, I shall be obliged to you, or any of your correspondents, to inform me.

I am, Sir,
Your obedient servant,
J. G——.

REPLY.

Method of taking transit observations.

It is certainly not difficult to observe to tenth parts of a second; and of this my correspondent will easily satisfy himself by trial with a common watch of five beats in a second. A phenomenon, as for example, the transit of a star, may take place in any one of the five beats, or between any two of them. If the observer repeat the words (either mentally or otherwise) *One*, one, one, one, one—*Two*, two, two, two, two—*Three*, three, three, three, three, &c., at each beat of his seconds clock, the word in Italic at the very beat, he will be enabled to mark the fractions of seconds with great precision. Musicians, in the rapid execution of prestissimo movements, divide the second still lower. As to the hundredth parts of seconds, though it might by some expedients be practicable to observe them, this is not implied in Astronomical Tables. They are almost always the results of means taken between a number of observations; and the second decimal may be considered as

as indicating the precise value of the first, instead of the sign + or —, which is sometimes annexed for the like purpose.

W. N.

XIII.

Examination of the Root of Calaguala: by Mr. VAUQUELIN *.

THIS root has a brown colour and a wrinkled surface in consequence of dessication. In some parts it is covered with scales like those found on the roots of common ferns. It is hard, coriaceous, and difficult to powder. It appears to be the root of a species of polypody. External appearance.

Exp. 1. Thirty grammes (463 grains) of this root coarsely powdered were digested in three hundred grammes of distilled water for forty eight hours. The water acquired very little colour, but it had a degree of consistence and unctuosity, so that it would not easily pass the filter. Its taste was slightly saccharine. Digested in water.

The infusion having been mixed with different reagents, the following effects were produced in it. Action of reagents on the infusion.

1. By alcohol was thrown down a yellowish white flocculent precipitate.

2. With sulphate of iron it assumed a blueish green colour, but without any perceptible precipitation.

3. With acetite of lead a very copious yellowish white precipitate was produced.

4. Oxalate of ammonia occasioned a very light precipitate in it.

5. No precipitate occurred on the addition of nitrate of barytes, infusion of galls, or solution of animal gelatine.

6. Lastly it was slightly reddened by infusion of litmus.

The effect of alcohol teaches us, that it contains a mucous substance: that of sulphate of iron, that it contains a resin similar to those of cinchona, of rhubarb, &c.: that acetite of lead indicates an acid, which may perhaps be the malic: Inferences from these effects,

* Annales de Chimie, vol. LV, p. 22.

of

of oxalate of ammonia, a little calcareous salt. The nitrate of barytes proves, that it contains no sulphuric salt: the galls, that it has no animalised substance: the solution of isinglass, that no tannin is present. The infusion of litmus shows the presence of some acid.

corroborated by farther experiments.

The following experiments, to which I was led by the foregoing, will demonstrate by their results the existence of most of the principles indicated above.

Digested in alcohol.

Exp. 2. Thirty grammes of the same root were digested forty eight hours in about 200 grammes of alcohol. This liquid assumed a deeper colour than the water employed in the first experiment. Its taste was at first saccharine, but it left behind a very strong sensation of bitterness.

Precipitated by water.

On the addition of water it became slightly milky, which confirms the existence of the resin mentioned above.

Distilled.

This tincture subjected to distillation till it was reduced to six or seven grammes, afforded a certain quantity of oil of a deep red colour, which was precipitated to the bottom of the liquid. The supernatant fluid had then not so deep a colour, and a less bitter, but more saccharine, taste. These effects were owing to the separation of the resin by the evaporation of the alcohol; and to the fluid remaining as less volatile holding in solution the saccharine matter.

Residuum.

As a little alcohol still remained in the fluid, which retained some of the resin in solution, I evaporated almost to dryness with a gentle heat. I then washed the residuum with a little distilled water, which enabled me to separate the saccharine matter pretty accurately from the resin. The alcohol that had come over had not carried with it any sensible portion of oil, for it was not rendered turbid by the addition of water; but thus mixed it had a peculiar smell, and an acrid taste.

Resin.

The resin separated from the saccharine matter in the manner above mentioned had a brownish red colour, a very strong acrid and bitter taste, and was soluble in alkalis, to which it imparted a brown colour and considerable bitterness. Acids decompossed this alkaline solution, and separated the resin just as it was before.

Probably destroys the tape-worm.

Is not this resinous substance, which ought equally to be found in the other species of ferns, the principle that destroys

destroys the tape-worm? This is not improbable, for we know, that all acrid and caustic oils produce this effect.

The saccharine substance, which had been dissolved by the alcohol at the same time with the resin, gives a slight lemon colour to water. It is reduced to a thick and viscous substance by evaporation. Its taste is sweet, pleasant, and slightly acid. Its smell is nearly similar to that of the juice of apples when evaporated. On being heated it swells up, grows black, and emits a smell exactly resembling that of burned sugar. I found in it perceptible traces of muriate of potash. Thus it appears there can be no doubt, that this substance is a true sugar, with which an acid, probably the malic, is mixed; but of this I could not satisfy myself by experiment, the quantity being too small. Saccharine matter.

Exp. 3. To obtain those principles of the calaguala root, which are not soluble in alcohol, I digested in water for forty eight hours that portion of the root, which had already been treated with alcohol, as has been seen. The colour it imparted to the water was deep, as if it had given out nothing to alcohol. This infusion had no bitter taste, like that in alcohol: it frothed when shaken; it precipitated solution of silver pretty copiously in a substance which had all the appearance of muriate of silver. Evaporated in a gentle heat, it left an extract of a brown yellow colour, transparent, very tenacious and stringy, on which spirit of wine had no effect. This extract had a mucilaginous and slightly nauseous taste: mixed with a little sulphuric acid it grew black, and exhaled copious fumes of muriatic acid: put on a redhot iron it swelled up, and emitted a smell similar to that of gums. This matter then appears to be nothing but a mucilage coloured by a small quantity of extractive matter insoluble in alcohol, and mixed with a certain quantity of a muriatic salt, probably with potash for its base. Root digested in water, after being treated with alcohol. Properties of this infusion.

Exp. 4. The root of calaguala thus successively exhausted by alcohol and water I afterward treated with weak nitric acid, in order to know whether it contained any amylaceous matter. After two days digestion with a gentle heat, I filtered the liquid, which had acquired only a slight amber Residuum treated with nitric acid.

amber colour, while the root had become of a pretty bright red.

An alkali added.

An alkali mixed with this fluid precipitated nothing; but it produced in it a very lively and agreeable violet red colour. The filter too, through which I passed this nitric infusion, assumed on drying a pretty fine red.

Precipitated with alcohol.

The same nitric infusion, being mixed with four parts of alcohol, yielded a light flocculent precipitate of a very fine white colour, which, when separated from the supernatant fluid, and washed with fresh portions of alcohol, redissolved in cold water. This substance had all the appearance of common starch, that had been dissolved in nitric acid, and afterward precipitated by alcohol: but I had not a sufficient quantity, to satisfy myself that it was so in a positive manner. At least there is every reason to believe, that it is not gum, otherwise it would have dissolved in water, and furnished some traces of mucous acid on being treated with nitric acid; but I obtained from it only the oxalic. The nitric acid then, according to all appearance, took up from the calaguala root a certain quantity of amylaceous matter, and a colouring substance insoluble in alcohol, which alkalis turn to a violet.

The residuum, $\frac{4}{5}$ of the whole, incinerated.

The calaguala root treated by the different reagents mentioned above, and afterward dried,, had lost a fifth of its weight. All that remained was the woody part, and the earths insoluble in acids. To ascertain the nature of the latter, and pretty nearly their quantity, I burned the residuum in a crucible till it was completely incinerated; and from about twelve grammes of the root, I obtained half a gramme of ashes, which were composed of carbonate of lime, that the nitric acid had not dissolved, and certainly did not exist in that state in the root itself, with a small quantity of muriate of potash, and some traces of silex.

The root treated with the same menstrua in a different order.

I treated the calaguala root a second time with the same menstrua, but in an inverted order, beginning with water, next employing alcohol, and finishing with nitric acid. By the first operation I obtained the sugar, the gum, part of the salts, and a little colouring matter. By the second I got the resin, and a little of the sugar, that had escaped the

the action of the water. Lastly by the third I dissolved the amylaceous portion, and the peculiar colouring substance I have mentioned above.

On recapitulating all the products obtained by the different operations mentioned in the course of this paper, we find, that the root of calaguala is formed of

Component parts of the root.

1. A large quantity of woody matter:

2. A gummy substance, which comes next in point of quantity:

3. A red, bitter, acrid resin, the next in proportion:

4. A saccharine matter, tolerably abundant:

5. An amylaceous part, the quantity of which I did not ascertain:

6. A colouring matter soluble in nitric acid, and turning violet on the addition of an alkali.

7. A small quantity of acid, which I could not discriminate, in consequence of its being so little, but which I suspect to be the malic:

8. A tolerably large quantity of muriate of potash:

9. Lastly lime and silex.

Medicinal parts.

Of all these substances those soluble in water and alcohol are alone capable of producing any effect on the animal economy. These substances are the sugar, mucilage, muriate of potash, and resin.

Roots of male fern and common polypody contain the same principles and tannin.

Since the time when I analysed this root at the request of Mr. Alyon, I have subjected to similar experiments the roots of common polypody and the male fern, and obtained from them precisely similar principles nearly in the same proportions as from the calaguala root. The former roots however contain a small quantity of tannin. Thus the analogy of organization, which led Mr. de Jussieu and Mr. Richard to conclude, that the medicinal virtues of the calaguala root must be similar to those of other ferns, is fully confirmed by chemical analysis.

XIV.

On the Chemical Nature of the Smut in Wheat. By Messrs. FOURCROY *and* VAUQUELIN *.

Smut has already been examined imperfectly.

THE smut in wheat has already occupied the attention of several chemists. Parmentier has found in it a fetid, fat, and coally substance. Cornet has observed its oleaginous nature. Girod-Chantrans, in 1804, announced, that it contained also a free, fixed acid, which he supposed to be of a peculiar nature.

This discovery, announced to the Institute in the autumn of that year, induced Mr. Vauquelin and me to undertake a full examination of this degenerated vegetable matter.

Described.

It is well known, that the smut is in fact a corruption of the grain, which exhibits within the husk of the seed, instead of a farinaceous substance, a black, greasy, stinking powder, the most decided and dangerous characteristic of which is its being capable of infecting other grains by contact, and imparting to them the property of propagating smutty wheat. It is known too, that washing with lime and alkalis is the most certain method of removing its contagious property, and preventing the disease from being reproduced, which it constantly is, if this practice, now generally employed by all judicious farmers, be neglected.

Prevented by washing with alkalis.

Smut

The smut, on which we made our experiments, was given us by Mr. Girod-Chantrans.

treated with hot alcohol,

Triturated in an agate mortar, and separated from the husk, the smut imparted to hot alcohol a yellowish green colour; and, without communicating to it any character of acidity, exhibited only about a hundredth part of its weight of a deep green oily matter, as thick as butter, and acrid as rancid grease.

ether,

Ether separated from it the same oil.

and water.

After this action of alcohol, the smut retained both its greasy feel, and filthy smell. Lixiviated with five times its

* La Revue Philosoque, &c. Nov. 1805. Abridged from a paper read at the National Institute.

weight

weight of boiling water, it gave it a brown red colour, a fetid smell, a soapy quality, and a very decided acidity.

This acid, examined by various appropriate reagents, exhibited all the properties of the phosphoric. Acid appeared to be the phosphoric.

On lixiviating pure smut, not previously treated by alcohol, with boiling distilled water, this liquor, which was perceptibly acid, being saturated with potash, gave a precipitate of animal matter, mixed with crystallized ammoniaco-magnesian phosphate, and every proof of an alkaline phosphate. These experiments therefore confirm the existence of free phosphoric acid in smut, known by its fixedness, its insolubility in alcohol, its solubility in water, its precipitation by lime, &c. This confirmed.

After the aqueous infusion had been precipitated by potash, it held in solution a fetid animal matter, resembling in colour, smell, and the phenomena exhibited by its precipitation with various reagents, that found in water in which the gluten of wheat has putrefied. Animal matter resembling that from putrid gluten.

After having undergone the action of alcohol and water successively, the smut of wheat still retained both its fetid smell and greasy feel. Distilled on an open fire it afforded a third of its weight of water impregnated with acid acetate of ammonia; nearly a third of a deep brown, concrete oil, much resembling adipocere in its form, consistence, and fusibility by a gentle heat; and 0·23 of a coal, which, being incinerated, left 1 gramme [15½ grs.], being a hundredth part of the original smut, of white ashes, three fourths of which were phosphate of magnesia, and one fourth phosphate of lime. The residuum distilled.

We examined the smut with its husk, to compare it with that which had been deprived of it, but we did not find difference enough to ascribe to the bran that covers it any decided influence on its analysis. Smut examined with the husk.

From our examination, the leading results of which have just been given, we conclude, that the smut of wheat contains, Its component parts.

1. A green, butyraceous, fetid, and acrid oil, soluble in hot alcohol or ether, composing near a third of its weight, and imparting to it its greasy consistence. Oil.

2. A vegeto-animal substance, soluble in water, insoluble in Vegeto animal substance.

in alcohol, and precipitating most of the metallic salt, as well as galls. It composes rather less than a fourth of the smut, and is perfectly similar to what comes from putrified gluten.

Coal. 3. A coal, amounting to one fifth of its quantity, which gives a black colour to the whole mass; and is an evidence, as it is the product, of a putrid decomposition; a part which it acts equally in mould, and in all the remnants of putrified organic compounds.

Phosphoric acid. 4. Free phosphoric acid, scarcely constituting more than .004 of the smut, but sufficient to impart to it the property of reddening blue vegetable colours.

Phosphates. Lastly the phosphates of ammonia, magnesia, and lime, in the proportion of a few thousandths only.

A residuum of grain destroyed by putrefaction. The smut of wheat then is nothing more than a residuum of the putrified grain, which, instead of its original component parts, starch, gluten, and saccharine matter, exhibits only a kind of carbonaceous oily substance, very analogous to a kind of bitumen of animal or vegeto-animal origin.

Putrified gluten exhibits similar results. We must here remark, that in our examination of gluten decomposed by putrefaction, we found characters very similar to those of the smut of wheat; and that the products of the one are so like those of the other, as to render it difficult in certain cases not to confound them together. It requires a man to be well practised in chemical experiments, to discern the slight differences, that exist between these two putrified matters, because these differences consist only in delicate shades, that are not easily perceivable.

Still we are ignorant of its cause. Interesting as the results of this analysis may appear, we must confess, there is still a great distance from the knowledge they give us of its nature to that of its cause; and yet more to that of its contagious quality, which is proved by so many experiments, as to leave no room for the slightest doubt. We must own too, that these results, while they indicate the smut to be the residuum of putrified farina, do not entirely agree with the ideas of philosophical agriculturists, who consider this disease as the necessary product of contagion; since it thus seems natural to presume it arises from putrid decomposition, which may proceed from any other circumstance as well as a communicated germe.

May arise without contagion.

Attacks the gluten. The same results lead us equally to infer, that the putrescency,

trescency, which necessarily precedes the formation of the smut in all cases, whether it depend on contagion or arise spontaneously, attacks particularly the gluten; and precedes, indeed prevents, the formation of the starch: since we know positively, that this fecula, no traces of which are found in the smut of wheat, suffers no alteration from that septic process, which so powerfully attacks the glutinous substance.

XV.

Of the Action of Nitric Acid on Cork; by Mr. CHEVREUL*.

BRUGNATELLI having examined the action of nitric acid on cork, in 1787, found, that the cork was converted into a peculiar acid. In 1797 Bouillon-Lagrange resumed the inquiry of the Italian chemist, and confirmed the existence of the suberic acid. In the two papers he published on this subject, he described the characters of this acid, and its combinations with the salifiable bases, which Brugnatelli had not studied. Notwithstanding these labours, several persons still entertained doubts of the existence of this acid. They thought, that it was only one of the acids previously known combined with some matter, by which its properties were concealed. Of the truth of this I was desirous to satisfy myself by experiment. History of the discovery of suberic acid.

To form suberic acid, I followed the common process; which I shall here recite, with the phenomena that occurred in the operation. Preparation of the suberic acid.

In a retort, to which a receiver was adapted, I heated six parts of nitric acid at 29° on one of rasped cork. The matter grew yellow; nitrous gas mixed with carbonic acid was evolved; and a pretty large quantity of prussic acid was formed. I returned the product from the receiver into the retort several times, that the cork might be acted upon sufficiently. When the action of the acid appeared to

* Annales de Chimie, vol. LXII, p. 323.

abate,

abate, I poured the matter still hot into a porcelain capsule, where I finished the evaporation with a gentle heat, stirring it continually. As soon as it was reduced to the consistence of an extract, I put it with some water into a large glass phial on a sand heat. At the end of a few hours I withdrew it from the fire; and on cooling two solid substances separated. One of these, which I shall call A, sunk to the bottom in the form of large flocks: the other, B, congealed on the surface of the liquor like wax. This I removed with a piece of card; the other I separated by filtration.

Examination of the matter A.

The flocculent precipitate, A, was insipid; insoluble in water and in alcohol; and of a white colour, but turning a little brown on exposure to the air. Nitric acid at 32° did not act on it perceptibly. Placed on a red hot coal, it burned without swelling up, and emitted a pungent smell of empyreumatic vinegar. Its coal was bulky, and pretty hard. This substance therefore was nothing but the woody part naturally contained in the cork.

Examination of the matter B.

The supernatant substance, B, had very little taste. It was insoluble in water; but boiling alcohol dissolved it, some portion of woody matter excepted. The filtered solution on cooling let fall a white substance resembling wax. This being separated by a second filtration, I added water to the solution, which threw down a straw coloured resinous substance, that turned reddish by exposure to the air, and was acid, notwithstanding I had washed it repeatedly. On distillation it yielded a sort of concrete fat, and a very acid fluid, that precipitated acetate of lead. I could not ascertain its nature from the smallness of its quantity.

The water that had been employed to precipitate the resin acquired a yellow colour by evaporation, and a taste resembling that of bitter almonds. It contained only a little of the yellow matter, and probably a few atoms of prussic acid.

Examination of the fluid separated from the matter A.

The fluid from which the matter A had been separated had an acid and bitter taste; precipitated lime water and calcareous salts; turned solution of indigo green; contained a little iron, as appeared on the addition of galls; and, when the excess of acid was saturated, it did not precipitate gelatine,

gelatine, consequently contained none of the tannin of Mr. Hatchett.

In evaporating the fluid with a gentle heat, it emitted a pretty decided smell of vinegar. This induced me to finish the evaporation in a retort; but I obtained only nitric acid, without any acetous. Whether this were dissipated at the commencement, or its quantity were too small for me to detect it, I cannot say. The liquor, after evaporation and cooling, let fall an *acid sedimental matter*, which I separated by filtration. Four successive evaporations afforded me fresh acid. After the fifth evaporation I obtained crystals of oxalic acid. Having decanted the mother water, which was yellow, and had a very bitter taste, I precipitated the oxalic acid it still retained by lime water in excess, and distilled it. The liquid that came over into the receiver contained a little ammonia. I then precipitated the liquid left in the retort by carbonate of potash, and lime was thrown down. The filtered liquor yielded in a couple of days some small gold coloured crystals of the bitter yellow matter combined with potash *. Suberic acid.

This acid sedimental matter was the suberic acid. I washed off with cold water part of the yellow matter that coloured it, and completed its purification by repeated solution in boiling water, from which it separated by cooling in little white flocks. By concentrating the bitter waters I separated that, which they held in solution. By this process I obtained a very white acid, about five parts of which were obtained from sixty of cork.

The suberic acid is as white as starch. It has an acid taste, without any bitterness. Light does not alter its whiteness. To dissolve one part of this acid requires 38 parts of water at 60° [140° F.], and 80 parts at 13° [55½° F.]. Its little solubility prevents us from having it crystallized; so that when it is dry it is always pulverulent and opake. Properties of the suberic acid.

* Having saturated the mother water of these crystals with muriatic acid, I obtained a precipitate, which exhibited all the characteristics of benzoic acid: but I dare not venture to assert, that this acid is constantly formed, for in three operations on cork I obtained it but once, and then in a very small quantity.

Thrown

Volatilizes.

Thrown on hot coals it is volatilized, without leaving any residuum, and emitting a smell of suet.

Heated in a retort.

When heated in a small glass retort on sand, it melts like fat with a gentle heat. If the retort be withdrawn from the fire, and the melted acid diffused over its inside, it crystallizes in needles by cooling. If the distillation be continued, it rises in vapours, which condense in the summit of the retort in white needles, some of which are half an inch long. This sublimate has all the characters of suberic acid. A slight coally mark is left in the retort.

Solution in water.

Suberic acid dissolved in water reddens litmus very distinctly. It does not preceipitate lime *, strontian, or barytes water, or the saline combinations of these bases. On evaporating lime-water saturated with suberic acid, the calcareous suberate falls down in a white flocculent precipitate, from which muriatic acid separates the suberic. This is indeed an excellent method of obtaining it perfectly white. The muriate of lime may be separated from it, by dissolving it in a small quantity of hot water; when by cooling we obtain the acid, which is always in a pulverulent form †, and similar to what it was before being combined with the lime; only this base takes from it the remains of the colouring matter, which the water had not dissolved.

Method of purifying.

Mistake of Brugnatelli.

* It seems to me, that Mr. Brugnatelli must have deceived himself, when he says, that suberic acid precipitates lime water, and all the mineral calcareous salts. The oxalic acid, which no person has mentioned, and which is formed with the suberic acid, was no doubt the occasion of the precipitate he obtained. It appears to me also, on reading the article suberic acid in Brugnatelli's Elements of Chemistry, vol. II, p. 106, that the acid he describes still retains bitter matter, resinous matter, and oxalic acid.

† I made this experiment, in order to see whether the suberic acid were analogous to the benzoic, and, in this case, to separate it from the matter that prevented its crystallization.

Purification by barytes.

I repeated the same experiment with barytes instead of lime, and had the same result. The suberate of barytes is deposited by concentration, and its decomposition by muriatic acid afforded me the suberic acid perfectly white. A small excess of the acid should always be employed, in order to separate the last portions of base, which the suberic acid might retain.

Ammonia

Ammonia and the fixed alkalis dissolve suberic acid very well. These combinations, when concentrated, let fall their acid on the addition of sulphuric acid, muriatic, &c. Action of alkalis.

The suberate of ammonia precipitates the solution of alum, and the nitrate and muriate of lime. But to obtain precipitates with the latter concentrated solutions must be employed, for the suberate of lime is pretty soluble. Suberate of ammonia.

Suberic acid throws down a white precipitate from a perfectly neutral solution of silver, from muriate of tin at a minimum, from sulphate of iron at a minimum, from nitrate and acetate of lead, and from nitrate of mercury. It does not precipitate sulphate of copper * or of zinc. Action of the acid on the metals.

Suberate of ammonia decomposes almost all the metallic solutions. The cupreous salts are precipitated by it of a pale blue; the cobaltic, rose-coloured; those of zinc, white; &c. Action of the suberate of ammonia.

Nitric acid has no action on the suberic. I boiled twelve parts of the former at 32° on one of the latter, without having any sensible decomposition. The suberic acid was dissolved, and this solution, being boiled down, deposited suberic acid some hours after cooling. I observed, that the addition of water promoted this separation. I thought at first, that I might obtain crystals from this acid solution, but I could not succeed. Nitric acid does not act on it.

Alcohol dissolves the suberic acid very well. When saturated with it, water precipitates a portion. Soluble in alcohol.

The suberic acid does not turn green the solution of indigo in sulphuric acid. Mr. Bouillon-Lagrange however lays much stress on this property, which he considers as a characteristic of the acid; and in fact if this change of colour were owing to a chemical action, it would be very surprising, that a substance formed amidst nitric acid should not have attained its complete oxidation, but remain capable Mistake of Bouillon Legrange.

* Bouillon Lagrange says, Ann. de Chimie, vol. XXIII, p. 48, that the suberic acid decomposes nitrate of mercury, and the sulphates of copper, iron, and zinc; and p. 56, that the suberic acid yields mercury and zinc to the three mineral acids, and iron and copper to sulphuric acid; which appears to me contradictory. Mistake of Bouillon Legrange.

ble of deoxiding indigo. Mr. Bouillon-Lagrange has ascribed to the suberic acid a property, that belongs to the bitter yellow matter, which forms a green by mixture with the blue of the indigo. It is this too, that turns a solution of copper green; for I have satisfied myself, that the white acid merely dilutes the blue colour, just as an equal quantity of water would have done.

Analogous to the sebacic acid.

From what has been said I conclude, that the suberic acid has great analogy with the sebacic, with which Mr. Thenard has made us acquainted *; and that the only striking difference between them is the crystalline form, which the suberic acid assumes when dissolved in water or in alcohol.

XVI.

Method of Fabricating artificial Stone employed in the Vicinity of Dunkirk. By Mr. BERTRAND, *Apothecary to the Army of the Coast*†.

Method of making artificial stone in France.

THE materials employed for this purpose are the ruins of the citadel, consisting of bricks, lime, and sand. These are broken to pieces by means of a mill, formed of two stone wheels, following each other, and drawn by a horse, Water is added; and the matter, when well ground, is reddish. This is put into a trough, and kept soft by means of water.

When the trough is full, some lime is burned, and slacked by leaving it exposed to the air, and this is mixed in the proportion of one eighth with the cement above.

A wooden mould is laid on the stone, and after a thin layer of sand is thrown on the stone, to prevent the cement's adhering to it, a layer of cement is poured in, and on this a

* See Journal, vol. I, p. 34.

† Annales de Chimie, vol. LV, p. 285.

layer

layer of bricks broken into acute-angled fragments. Thus two other strata are put in, before the last, which is of pure cement. The mould being removed, the stones thus formed are laid in heaps to dry. The lime being very greedy of water, and quickly becoming solid, these stones are not long in forming a hard body fit for building. Method of making artificial stone in France.

The lime is not very dear, being burned with pitcoal. The labour is not dear, requiring only one strong man assisted by two or three boys of twelve years old. The materials, being from old ruins, are cheap: and only one horse is employed in this manufactory, which is not the only one in the country. I believe others exist in Prussian Poland where these stones are made with much more success, because fragments of basaltes, which are better adapted to form a solid body with lime and alumine, are there used.

The pebbles of Boulogne would be still preferable, and I doubt not with these artificial stone might be made equal to natural stone in goodness.

XVII.

Letter from Mr. LINK, *Professor of Chemistry at Rostock, to Mr.* VOGEL*.

I HAVE just examined the pollen of the hazel nut. It differs greatly from that of the date tree, which Messrs. Fourcroy and Vauquelin have analysed. It contains a large quantity of tannin, a resin, a great deal of gluten, and a little fibrin. There is animal matter therefore in this pollen. Pollen of the hazel nut.

To learn the properties of the membranous part of plants, I subjected to research the pith of elder, and procured from it by nitric acid every thing, that Bouillon-La- Pith of elder.

* Annales de Chimie, vol. LXII, p. 292.

grange

grange obtained from cork, but without this substance leaving any residuum.

Suberic acid characteristic of vegetable membrane.

As Mr. Brugnatelli obtained suberic acid from paper, I believe it is a peculiar characteristic of vegetable membrane, to furnish this acid.

Crystals in the root of tree-primrose.

In the roots of the *œnothera biennis*, broadleaved tree-primrose, I have seen by the help of a good microscope extremely small crystals, regularly formed, accumulated in the cellular texture. It was difficult to obtain a sufficient quantity for a chemical analysis. They appeared to me somewhat analogous to the crystals obtained from indigo by Nicholson: they are very little, if at all, soluble in water, alcohol, or many of the acids; sulphuric acid itself acts but very feebly on them; the nitric acid alone is their true solvent.

Muriate of silver not blackened without light.

I have endeavoured to blacken the muriate of silver by a current of air employed in the dark, but found it impossible to succeed.

Berthollet's hypothesis.

Mr. Berthollet, as I see in his work, was able to blacken it by a simple current of air. He says, that light acts upon this salt by taking from it a portion of muriatic acid. But how will this celebrated chemist account for the black colour, that muriate of silver assumes when covered with muriatic acid?

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Mineral strata of Clackmannanshire.

AT the meeting of this Society on the 8th of April, was read the first part of a Description of the Mineral Strata of Clackmananshire, from the bed of the river Forth, to the base of the Ochils, illustrated by a large and very accurate plan and section of those strata, done from actual survey,

vey, and from the register of the borings and workings for coal in Mr. Erskine of Mar's estate in that district; communicated by Mr. Robert Bald, civil engineer, Alloa. In this first part, Mr. Bald treated only of the alluvial strata. In continuing the subject, he is to illustrate it still farther by exhibiting specimens of the rocks themselves.

Insects near Edinburg.

Mr. Charles Stewart laid before the Society a list of the Insects found by him in the neighbourhood of Edinburgh, with introductory remarks on the study of entomology. It would appear, that the neighbourhood of Edinburgh possesses no very peculiar insects, and but few rare ones. The list contained about four hundred species; which, Mr. Stewart stated, must be considered as the most common, as they were collected in the course of two seasons only, and without very favourable opportunities. It was produced (he added) merely as an incitement to younger and more zealous entomologists.

At this meeting there were laid on the Society's table the first two volumes, 4to. of Count de Bournon's System of Mineralogy, with a volumn of Outlines; a present from the author.

Mineralogy of Clackmananshire.

AT a meeting of this Society on the 13th of May, the second part of Mr. Bald's interesting Mineralogical Description of Clackmananshire was read; giving a particular account of two very remarkable *slips* or *shifts* in the strata, near one hundred feet in depth, by reason of which the main coal field of the country is divided into three fields, on all of which extensive collieries have been erected.

Flora of Linlithgow.

The Rev. Mr. Fleming of Bressay laid before the Society an outline of the Flora of Linlithgowshire, including only such plants as are omitted by Mr. Lightfoot, or marked as uncommon by Dr. Smith. This, he stated, was to be considered as the first of a series of communications illustrative of the natural history of his native country.

Eels found in a subterranean pool.

Mr. P. Walker stated a curious fact in the history of the common eel. A number of eels, old and young, were found

found in a subterraneous pool at the bottom of an old quarry, which had been filled up, and its surface ploughed and cropped for above a dozen of years past.

Seasnake.

The Secretary read a letter from the Rev. Mr. Maclean, of Small Isles, mentioning the appearance of a vast sea-snake, between 70 and 80 feet long, among the Hebrides, in June, 1808.

Plants near Edinburgh.

And he produced a list of about one hundred herbaceous plants, and two hundred cryptogamia, found in the King's Park, Edinburgh, and not enumerated in Mr. Yalden's catalogue of plants growing there; communicated by Mr. G. Dow, of Forfar, late superintendant of the Royal Botanic Garden at Edinburgh.

Elementory treatise on Geology.

Mr. De Luc has in the press an *Elementary Treatise on Geology*, which will contain an examination of some modern geological systems, and particularly of the Huttonian Theory of the Earth. We understand, that this work is translated from the French manuscript of the Rev. H. De la Fite, M. A., and will form an octavo volume.

French Journals.

I HAVE just received some of the French Journals, that have been so long in arrear; and am informed, that the rest are on their way from Paris. From those that have come to hand I extract the following.

Potash in mica.

Mr. Klaproth has discovered in mica sixteen per cent of potash.

Turkois analysed.

Dr. John, of Berlin, has lately described and analysed an oriental turquoise from Bisiapoor, near Corasan, which he found to contain

Alumine	73
Oxide of copper	4·5
——— iron	4
Water	18
	99·5

This

This result verifies that of the late Lowitz. We have therefore two distinct species of the turquoise; and may give to this now mentioned Pliny's name of *calaïs*.

Dr. John likewise conceives, that he has found a new volatile and acidifiable metal in the grey ore of manganese from Saxony. He obtained it by distilling the ore with sulphuric acid. The volatile metallic acid combines with a weak solution of potash put into the receiver, and tinges it crimson. From this red liquor gallic acid, or infusion of galls, throws down a chesnut brown precipitate. Prussiates immediately change the red colour to a fine lemon yellow, but without any precipitation. The carbonates do not precipitate the red solution; but if it be heated with a little alcohol, the red colour changes to a green, a smell of ether is given out, and then the carbonates throw down a brown oxide, which is soluble in muriatic acid. New metal.

Mr. Bucholz has found, that the schorliform beryl of Bavaria is a true beryl containing 0·12 of glucine. Bavarian beryl.

Mr. Braconnot has analysed some fossile horns of an extraordinary size found in an excavation at St. Martin, near Commercy. He supposes these to have been the horns of the great wild ox, the *urus* of the ancients, *aurochs* of the Germans. From a hundred parts he obtained

Analysis of some fossil horns.

Ferriferous quartz sand		4
Solid gelatine		4·6
Bituminous matter		4·4
Oxide of iron		0·5
Alumine		0·7
Phosphate of magnesia		1
Water		11
Carbonate of lime		4·3
Phosphate of lime, composed of		
Phosphoric acid	28·3	69·3
Lime	41	
		100·

To CORRESPONDENTS.

Mr. Ibbetson's and Mr. Rootsey's Papers, and Mr. Thompson's Analysis of Sulphate of Barytes, will appear in our next number.

Meteorolo-

METEOROLOGICAL JOURNAL

For MAY, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

Day of APR.	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
23	43	41	46	40	30·01	Rain	Cloudy
24	41	43	48	38	30·28	Fair	Ditto
25	40	43	48	41	30·32	Ditto	Ditto
26	45	44	51	43	29·92	Rain*	Rain
27	48	48	53	46	29·71	Ditto	Cloudy
28	49	47	52	40	29·41	Ditto	Rain
29	42	41	46	35	29·67	Fair	Fair†
30	41	45	50	40	29·75	Ditto	Cloudy
MAY							
1	42	42	50	38	29·35	Rain	Fair
2	43	43	49	37	29·46	Hail ‡	Ditto
3	42	45	52	41	29·80	Fair	Ditto
4	46	44	52	42	29·93	Rain	Ditto
5	48	48	55	40	29·96	Fair	Cloudy
6	50	53	57	50	30·22	Ditto	Fair
7	53	58	64	50	30·32	Ditto	Ditto
8	54	58	64	48	30·30	Ditto	Ditto
9	55	59	65	49	30·22	Ditto	Ditto
10	57	59	67	52	30·08	Ditto	Ditto
11	59	61	70	56	30·00	Ditto	Ditto
12	62	64	72	55	30·00	Ditto	Ditto
13	64	63	71	56	30.00	Ditto	Ditto
14	64	62	72	56	29·96	Ditto	Ditto §
15	66	58	68	55	29·86	Rain	Ditto
16	65	64	70	57	29·79	Fair	Ditto
17	64	65	73	60	29·86	Ditto	Cloudy ‖
18	65	70	72	63	29·82	Ditto	Rain
19	65	68	72	55	29·60	Rain ¶	Ditto
20	58	58	61	51	29·79	Ditto	Fair
21	55	57	61	50	29·93	Fair	Ditto
22	54	54	63	53	30·18	Ditto	Ditto
23	58	55	66	50	30.24	Fair	Ditto
24	56	55	65	51	30·21	Ditto	Fair
25	53	53	62	51	30·09	Ditto	Ditto
26	54	51	63		29·90	Ditto	Air chilly, with rain

* The whole day.

† Too cloudy at 11 and afterward, to observe the eclipse.

‡ Hail at 11 A. M., lightning and thunder at 1 P. M.

§ Lightning at 11 P. M. ‖ At 10 high wind with lightning—sultry hot.

¶ In the afternoon tremendous thunder and lightning with heavy rain.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1809.

ARTICLE I.

On the Impregnation of the Seed, and first Shooting of the Nerve of Life, in the Embryo of Plants. In a Letter from A. IBBETSON, *Esq.*

SIR,

FOR many years botany and the study of the anatomy of plants have been my favourite occupation in solitude, nor had I any intention to subject that, which was undertaken only as a recreation, to the notice of the public: but some curious details having occurred, which appear to me not well known, if you think them worthy a place in your excellent Journal, they are at your service.

The very exact description that has been given by many intelligent botanists of the growth of the infant plant, from the time the seed is ripe for the Earth, renders it unnecessary for any one to repeat, what has been so well detailed; but there are curious particulars, *preceding* this time, of which little is said, and still less understood; which I have long

Difficulties in the study.

long made my particular study, though I have had to encounter difficulties not a little discouraging, and in the investigation of which such patience is required, as would deter the most laborious students; beside the necessity of a most powerful solar microscope for opake objects; to which is added, improvements not generally applied, and which causes it greatly to excel in clearness of vision.

Impregnation of the seed.

The investigation I mean is, "The impregnation of the seed; and the first shooting of the infant plant, or rather of the germe or vessel which precedes it." It is almost impossible to ascertain the exact time when the seed is first formed in the pericarp. I have always found them in the winter bud, where there is any large enough for dissection. It is most curious to see the vessels, which may properly be called the life, tracing their way to each flower bud; for a seed may be said to depend for perfection on *two separate moments:* the one in which life *first enters the seed*, when the whole outward form appears to be perfected; and the second, when the impregnation of the seed takes place, by the ripening of the pollen, as I shall hereafter show. But when the life enters, it leaves a little string, and afterward remains a long time in a torpid state. This string crosses the corculum, or heart of the seed, so called because it is the cradle of the infant plant.

Outward form of the seed.

Two distinct organs attaching the seed to the seed vessel.

The seed is attached to the seed vessel by two distinct organs, which the first botanists have agreed to call the umbilical cord; but I think they are improperly so named, since they do not convey the nourishment to the infant plant, which is wholly the office of the *second set of vessels.*

The first is, I conceive, the life of the plant, since without it the plant dies, and with it *uninjured*, every other part may by degrees be eradicated, and will grow again. I have tried the experiment on many thousands, and never failed. These delicate simple vessels, carrying a juice of a particular nature, are to be traced in every part, lying between the wood and the pith. Nature has plainly shown their consequence, by denying them to the leaf bud; (and what gardener would take the leaf bud to *bud with? None;* for it possesses *not the life)* but Providence by a sort of instinct most curious teaches it to pass by the leaf bud, and proceed

to

to the female flower, where it establishes a new life in the seed. This life will enable it to grow, but not give life again, without impregnation. These vessels are the life therefore, from which all flower branches grow, and all root threads proceed. In calling it so, I only express what its office seems to denote. Hill traced it exactly, and called it the circle of propagation.

The next organ, that attaches the seed to the seed vessel, consists of the nourishing vessels. I am rather inclined to think, that these proceed from the inner bark: at least they may certainly be traced thence after the infant plant has left the seed. When introduced they enter not the seed at the same place as the life does; they come not into the corculum, but pass it, and spread themselves over a small spot below it, which is visibly of a different nature from the rest of the seed. In farinaceous plants it is yellower, and yields a milk white juice; but in other seeds it is whiter, and gives a glutinous water of a sweetish taste. Probably the vessels come from the fruit *filled with this juice*, which medicated with that part of the seed (which very apparently dissolves) they together form a nourishment suited to the infant plant.

The nourishing vessels.

Juices of each seed.

When the seed is so far perfected, it remains in an almost torpid state, or growing very little; while the flower expands daily, and the stamens are hastily advancing to their perfect state. It is now that beautiful process takes place, which, by an almost imperceptible contraction of the lower part of the pistil, raises the juice to the pointal, whence it may be seen hanging in a large glutinous drop, but which never falls. As soon however as the heat of the mid-day ceases, this juice, which is peculiar to the pistil, retires again within the tube, the contraction ceasing with the heat that caused it. This is continued each day, till the stamens are ripe, and ready to give out their interior powder; the greater part of which the pistil is always so placed as to receive; and as the pollen requires only *moisture* to burst it, it soon yields that fine and imperceptible dust, which quickly *melting* and *mixing with the before-mentioned liquid*, forms a combination of so *powerful* and *stimulating* a quality, that it no sooner runs down the interior of the

Contraction of the pistil.

The rising of the drop in the pointal

 style,

style, and touches the nerve of life in the heart of the seed; but *this vessel shoots forth* in the most surprising degree, forming directly a species of *circular hook* within *the void;* which in less than two days is often completely filled, though it had perhaps lain for many weeks before in an absolute torpor. This circular nerve is soon covered by an excrescence that hides it; but if the corculum is divided with a fine lancet, the circular hook is discoverable, till the young plant is near leaving its cradle or seed. At the turn of the hook the cotyledons grow, and the root shoots from the curved end.

fills the void when the stamens are ripe.

The plant may be now said to lie in the seed in a contrary direction from that in which it will at a future time grow, since the root is above, and the stem below: but Nature has provided for their change of place, since it is effected as they leave the seed. I have mentioned before, that the nourishment of the infant plant is medicated between the juice brought in the nourishing vessels, and the peculiar spot in the seed. This liquid continues to abound, indeed the infant plant may be said to *repose in it,* till the root has opened the whole, or part of the seed. The root then changes its direction, and runs into the earth, soon forming a number of stringy hairs, which serve as so many suckers to draw the liquid nourishment from the earth, while the plant quickly shows, by the rapid progress it makes, the advantage it receives from its change of diet; for it soon raises itself from its prostrate posture, emerges from the seed, and is now seen in its proper direction.

Change of posture in leaving the seed.

Nourishment of the plant.

Root strings pump up the nourishment.

I would not interrupt my account of the growth of the young plant, though my letter was written merely to detail the first steps, which are I believe unknown, but which confirm I think most thoroughly the sexual system, though some of the *syngenesian orders* give, if possible, a more convincing proof of it. The pistil runs up from the seed, being mostly single; and the juice of the pistil has no other way of reaching the pointal but passing through the seed, which it does without producing any effect, or filling up the vacancy at the top of the corculum. But no sooner does this same juice get mixed with the flower of the pollen, which dissolves in it, than the void becomes filled, the hook

Prove the sexual system.

hook is soon formed, and the young plant is raised to life.

They who doubt, that each part of the plant has its different juices, *proceeding from* and appertaining to the produce of one part alone; that is, the wood, when rising to the flowering part, gives its juice *only* to *form* the stamens; the line of life to form the pistil; the bark to form the corol, &c.; would no longer deny their assent, if they would dissect, and very much magnify the part of the pericarp just *above* and *below* the seeds, and see the extreme pains nature takes, that the juices may in no manner be mixed. I have drawings of almost every different *formed flower* in these parts, both *English* and *exotic;* and I think I could *prove* the truth of this assertion, without having recourse to the *rationalia* of the matter, which would certainly show the impossibility, that such parts, so different in their appearance, so opposite in their tendency, should grow from the same vessels, and proceed from the same juices. Nature gives us also a proof of the confusion occasioned by the mixture of the juices in the double flower, which owes its deformity probably to this cause only; as I have always found, on *dissecting* and *comparing* double and *single flowers of the same species together*, that, when it is the pistil that fails, the *style* is *discovered* to be *burst the whole way*, so that the juices can neither pass to the stigma for impregnation, nor return again to the seed: but when the stamens are imperfect, the seeds are often found in the pericarp, but they never have the void in the corculum *filled up*; and I have often seen the inner vessel of the style hanging like a useless thread in the middle of the seed vessel, and a confusion visible in every part, which seems to prove a general mixture of the juices, from the *excess* of *nourishment bursting* the *delicate* fibres, that contained each peculiar liquid.

Peculiar juices appropriate to each pait.

Double flowers owing to too much nourishment bursting the finer vessel, and mixing the juices, thus causing monsters.

I meant not however to enter into this digression, as it is a subject that requires many drawings to elucidate it, and more reasonings than a short paragraph will admit of. I return therefore to the infant plant, and shall venture to add a few of the innumerable experiments made to prove whether this cord of life (or as it is generally called *umbilical cord)*

The loss of the cotyledons does not kill a plant.

cord) is or is not the life of the plant. I placed a bean in the earth, and when the infant plant was ready to leave the seed; I opened it with a fine lancet, and cut off the cotyledons, just where they join the heart and the circular hook I have before described. Tying a piece of thread, easy to be broken, round the bean, I replaced it in the earth. The cotyledons grew again, though higher up, but they appeared very weak and sickly for some time.

The loss of the root does not kill it.

I then placed another bean in the earth, and at the same age I cut off the root. In a few days it grew again, and appeared perfectly healthy.

Throws out hairs to convey ourishment.

To see what the effect of taking away only the nourishing vessels would be, I separated and cut them off from each side of the bean; but the quantity of hairs, that grew from the wounded part, and attained the moisture to convey the nourishment, and supply the place of the part I cut away, is almost incredible.

Invariably dies with the loss of the nerve of life.

I now took a bean about four days in the earth, and opening it with great care, I took out with a fine lancet the part which I esteem the cord of life (See Pl. V, fig. 1, *l l*), that is, the part which crossed the corculum, and shot forth on the first impregnation of the plant. The whole decayed. I repeated this more than a dozen times, the plant always died.

I took a flower of the lilium species, as having a large seed vessel easily attained; and, being careful not to separate it from the nourishing vessels, I divided the line of life, cutting each thread between the seeds. Its seeds were never impregnated.

Infant plant killed by taking out the line of life.

Source of life in decayed trees.

I now tried the taking the nerve of life from the chesnut, the walnut, acorn, &c., first opening a seed *without touching* the nerve, that I might be assured that the opening was not the cause of its death. Those from which I took the nerve, all died; and the others, that I had merely laid open, lived. It is only at the first beginning of life, that the plant is to be killed by this process; when older, if the nerves decay, they shoot out above the declining part, and run into any part of the stem that is pure, to preserve themselves. This is the source of life in very decayed trees. This is the cause of a double pith, or at least of the appearance

ance of it, in many trees. This also in many grasses has a very particular appearance. I once found in the spring four yards of the poa trivialis with a root now and then, the whole dead; but on farther examining the plant, the end farthest removed from the root was beginning to shoot. On subjecting it to the solar microscope, I found the nerve of life had run in one diminutive string of vessels finer than a hair, of a bright green, and defended from the inclemency of the weather by the deadened part. As soon as the mildness of the season permitted, it shot forth; the rest of the parts were added by degrees, and the decayed fell off.

Double pith.

Poa trivialis.

I have many curious specimens of stems in which the vessels of life have been turned out of their natural situation: but it requires so many drawings to give a perfect idea of them, that of course such a work as yours could not admit them. I once traced these vessels from the stem to the apple, and thence to the line in the seed in one string; but this is extremely difficult to be done.

Vessels of life turned out of their natural situation.

I shall now conclude with noticing two extraordinary proofs of volition in some plants difficult to be accounted for by mechanical force only. I divided a bean into two pieces, and planted that half in which the young plant is found. In five days the stem had forced itself out at the usual place, but the root had taken a *shorter road*, and come out at the truncated part as more immediate to the earth. What mechanical power could occasion this difference? I took a bean in health, that had just quitted the seed, and cut off the root. The nourishing vessels had been dried up a day or two. I wrapped the truncated part in paper, fearing that it would throw out hairs to nourish itself, and then replaced it in the ground. How great was my astonishment to find, not only that the bean lived, but that the nourishing vessels had *reassumed their office* of supporting the plant! that the bean, which had been perfectly dry, was now as moist as in its earliest state, and continued to support the plant till the root had again grown, and forced itself through the paper! I have ever been an advocate for mechanical power, but can scarce reconcile these two instances to such a cause.

Proofs of volition in plants.

Nourishing vessels regain their office.

The various names given to the infant plant and its different

ferent parts have made me very unwilling to fix on an appellation, till it is ascertained what *are its parts and their uses*; as I cannot but imagine, that so many various appellations have the effect of making those that write unintelligible to one another, and much more so to those, who wish for information without much previous study. I shall add a little account of the names used to the sketch annexed, which will, I hope, make the parts easy to be comprehended.

Your obliged servant,

Bellevue, near Exeter. A. IBBETSON.

Explanation of Plate V.

Fig. 1. Representation of the bean. *o o* the nourishing vessels. *L* to *n* the seminal leaves, or cotyledons.

l to *l* the embryo: what I esteem the first shoot which the nerve of life makes, when it enters the corculum, or heart, which is more easily seen in the seed of the lily as at fig. 2, *l l*, where it crosses the empty part of the corculum as before explained.

When I took out the line of life in the bean, it was the two vessels within, from *l* to *l*. When in the lily, fig. 3, I merely divided the line *l*, preventing that communication from seed to seed, and not touching *o o*, which I think is the nourishing vessel, as may be seen at fig. 2, *o*, where they enter. Fig. 4 is the seed of the gooseberry, *o* the nourishing vessels, *l* the line of life, and *m* the corculum, or heart. Fig. 5 is the heart taken out of the seed of a chesnut. *l* is the circular hook, *o o* the nourishing vessels, and *l l* the line of life, which I took out where it crosses the heart at *m*. In almost every kind of seed it shows itself differently. In many it enters at or near the stalk, and runs under the albumen, or outward case. Having much more studied nature than botanical works; which indeed I began with, till I found that they inclined me to embrace a system, which I wished much to avoid; I have since trusted to nature only. I hope therefore to be excused the contradicting any one, as I may truly say I have not advanced a thing I have not tried

Fig. 1.

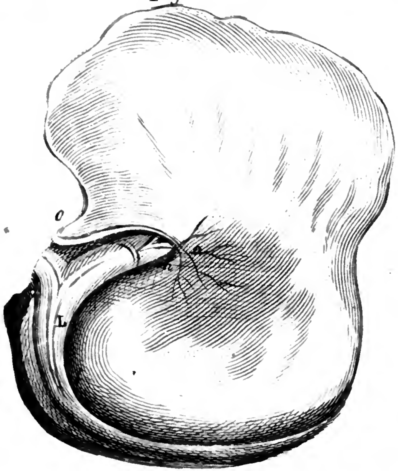


Fig. 4.

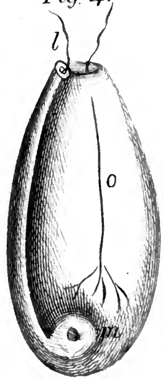


Fig. 5.

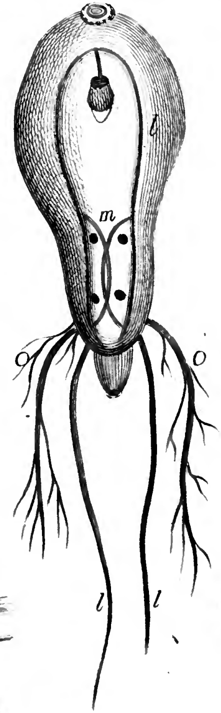


Fig. 6.

Fig. 7.

Fig. 3.

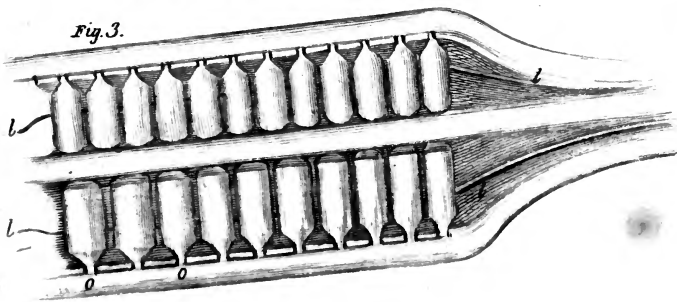


Fig. 8.

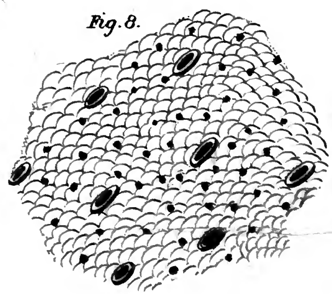
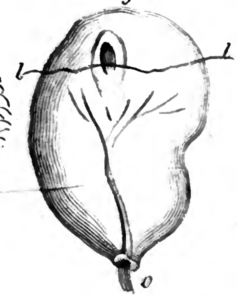


Fig. 2.



tried a number of times. Yet I am but too sensible how open I am to deception, and it is with a real feeling of humility I offer these opinions.

That the life of the plant is peculiarly resident in the vessels that run in a circular collection between the pith and wood, or medulla and liber, is most strongly proved by the manner in which all fruit is killed, if examined the morning after a sudden frost. It is *not* the *corolla*, the *calyx*, the *males*, or the *seeds* that are hurt; but the *female* is struck with *death*. And if the pistil is examined with care, it will be found, that it is the *line of life* which is decayed, and that this is the first part in which mortification commences. The peculiar liquor of the pistil turns to a blood red, and the vessels that run up to the pointal turn black. I have marked one at fig. 7 just taken from the tree, and killed in the last frost. The dark lines in fig. 7, which is dead, show the black and red vessels mentioned above; these being yellow in their natural state, which is delineated at fig. 6.

It is almost unnecessary to mention, that seeds must be examined in their first formation, to show the line of life; which, when once it has done its office, detaches itself. If the seed is boiled, the line of life and nourishing vessels will mark themselves by turning a dark colour. In very small seeds the mouth is often the best dissecter.

II.

On the Perspiration of Plants. *By* A. Ibbetson, *Esq.*

SIR,

As my first paper is short, I shall venture to join to it an incident, that has surprised me not a little, and that may perhaps from its novelty be acceptable to your readers. I have long entertained great doubts respecting the evaporation of plants; I mean not that insensible perspiration that will show itself by throwing a mist on the glass that covers it; but that which Bonnet insists on, and which Du Hamel weighed (which in 24 hours was double and treble the

Perspiration of plants.

the weight of the plant, even in a *sunflower*, which is *the heaviest* of plants); and my experiments have so fully answered my ideas respecting it, and *confirmed my doubts*, without however throwing the least blame on the very perfect experiments of these excellent botanists, that I shall have the greatest pleasure in offering you the result.

Doubts respecting it.

The constant habit of watching my plants at a very early hour in the morning, and examining them with very powerful microscopes, had almost convinced me, that the idea of their perspiring was a mistake; still, being acknowledged by such excellent botanists, it required the most *absolute* conviction, to gain courage to deny a fact so universally received as a truth. I rise at a very early hour, and had often observed, that, when there was no dew, the leaves remained perfectly dry, though examined with a powerful microscope; that when plants remained within doors, they collected dust like any other furniture; and that this dust was to be blown off with ease, neither agglutinating nor sticking, which it would do if partially wet: that, after placing a leaf for 4 hours in the opake solar microscope, though it was so placed as to be in its growing state, and was magnified so greatly as to show both species of pores, yet I could never see the smallest quantity of moisture exude, except what I shall now mention, and what I suppose may be the insensible perspiration before insisted on.

Insensible perspiration.

Almost every leaf, if subjected to a large magnifier, appears covered with a very fine scurf, which I have seen exude as water with the oxigen it is continually giving out, as long as the sun shines. In a very short time it turns to a jelly; which is, *I think*, received again into the same pores with the dews of the night; and which I doubt not helps to form that beautiful combination, which changes dead and unorganised matter into living bodies, fitted, as Mirbel beautifully expresses it, for the support of the animal creation. But this is so trifling a perspiration, that it will merely account for the dew, that appears when a vegetable is placed under a glass; but will not raise, or in a very slight degree only, the hygrometer placed within it.

Taken back.

This very trifling.

These doubts suggested the idea of investigating the matter more thoroughly, and I set on foot a number of experiments

periments, which I shall now detail, prefacing them with an observation which is necessary to begin with, because it is one of the signs given of perspiration, which I cannot *assent* to. Hales and Bonnet both observe, that, having placed a plant under a glass, the water after a time ran down or bedewed the glass. Put a wet sponge under a cylinder, and it will produce the same effect; and yet we should not say, that the sponge perspired, but that some of the moisture within the sponge had evaporated, and was condensed by the cold of the glass. In short it is merely a *sign*, that the *object* thus confined is *full of moisture*.

False sign of perspiration.

I shall now mention the experiments in the order in which I made them. I wished first to prove, which yielded most moisture, the *earth* or *plants*. I placed a small rose tree under a large glass in a pot of earth, placing at the same time Captain Kater's excellent hygrometer* with it, which then stood at 620 from excess of dryness. In 8 hours the moisture ran down the glass, and the hygrometer was at 1100, nearly excess of moisture. I then took away the rose tree, and, drying the glass, I put a pot of fresh earth the same size and weight, and with the same arrangement. In 8 hours the hygrometer, which had been put in at 616, came out 1049, or 433 more moist than it was when placed there. It was the *earth* therefore, that gave all that excess of moisture, not the plant.

Experiments on the rose tree; compared with fresh earth:

The next trial was made by fastening down a laurel branch, and passing it through a piece of sheet lead, without separating it from the tree; making it to fit a very large glass cylinder, then luting it round the lead, and at its entrance, to keep out the circulation of air, and prevent the wet vapour from passing upwards. After 8 hours the hygrometer was 130 nearer to dryness than when placed there, and though the glass was steamed, it did not run down with water; nor could I, with the largest magnifiers, *discover any dew drops* on the leaves.

on a laurel branch:

I now tried a vast number of plants with the same result, the hygrometer *never* showed an *increase*, which it would certainly have done, had the perspiration been so excessive;

and on various plants.

* For a description of this hygrometer, see our present number.

and

and it must have been perceptible on the leaves, but this was so far from the case, that the scurf before mentioned was not to be seen; it had certainly all settled in dew within the circumference of the glass.

Perspiration not perceptible.

I felt now perfect conviction, though not able to account for the *mistaken opinion* that prevailed, till walking one morning with my microscope in my hand, I found a pea plant covered with bubbles of water, and there had certainly been no dew. Here then was perspiration. I directly wiped off the drops, and covering the plant with a glass, treated it in the same manner as I had done the laurel, and many hundred of other plants. In a few hours it was again covered with bubbles of water, and the hygrometer indicating *extreme moisture*. I then tried a number of the same *genus*, but without effect, no *bubbles* were to be *seen*. I now concluded, that *some vegetables* did perspire, but that the numbers were *few*.

Perspiration of the pea.

Tried others of the genus but without effect.

Talking to a friend of the conviction I had gained, he intreated me to repeat a part of the experiments before him; I consented; and having first prepared the pea, in an hour or two it was covered with bubbles; but my friend not being yet arrived, I cut off the branch, and laid it on the table by me, fearful the bubbles would evaporate in the open air. In an hour I was surprised to see them turn of a milk white. I then applied to my solar microscope, and soon found, that the bubbles I had taken for *water* were a cryptogamian plant, having a regular stalk, which did not however raise it from the leaf, for it was so heavy it appeared *incapable* of *rising*. It lies like a long bubble, dies in a few hours, and is soon succeeded by a fresh set.

The supposed bubbles of water a cryptogamian plant.

No person could in its first state take it for any thing but water; indeed so completely did the bubbles resemble water, that the smallest touch broke the film which covered them, and their liquor was expended. Nor would any one believe it was not water, without seeing the stalk on which it grew, or without beholding its change of form. Its last state is an almost hard and long ball, which soon drops off. It is to be seen by a common little microscope; though stronger powers are required to view the whole process, especially the stalks. But so entirely does it cover the leaves,

This plant described.

that

that it doubles the weight of the plant, causes the hygrometer to indicate extreme moisture, and, confined under a glass, much of its liquor evaporates, condenses on the interior of the glass, and runs down on every side. I have since tried every plant specified as peculiar for their excessive perspiration by Bonnet, Hales, and others, and have found them all loaded with the cryptogamian plant, so that I have not the smallest doubt, that this was by them taken for perspiration; for what torrents of water would be necessary to supply such a *transpiration?* the air would be constantly loaded. The possibility of the mistake any person may convince themselves of, and how very likely it was to happen, by taking a pea plant, a sunflower, and a number of other plants unnecessary to mention.

Its liquor condenses on the glass,

and has been mistaken for perspiration.

I said, that leaves had *two species of pores*; the first *large*, which are open all the night for the admission of the dew; the second *small*, from which the oxigen flows. See Pl. V, fig. 8, representing part of a leaf sufficiently magnified, to show both sorts of pores. It is from the *smaller* that the jelly I have mentioned proceeds; for when the oxigen is saturated with moisture, it will naturally give it out in passing these narrow apertures, and this is that scurf which appears, when the leaves are not covered with a glass; but which flies upward, and is condensed on the interior, when they are.

Leaves have two kinds of pores.

I believe almost every air or gas has moisture, and that a full stream of oxigen directed against a glass would cover it with *steam*. I have just tried the experiment, and it has succeeded. It will of course depend upon its being nearly saturated with moisture, or not; and upon the pressure it afterward receives. I have endeavoured to condense my subject as much as possible, without I hope rendering it unintelligible. Should I see this in your first publication, it will serve as a hint to give you a farther letter on the formation of the leaf, and the winter bud. The latter certainly is of the first consequence in botany, and may be called the first source of life in the vegetable world.

Effect of a stream of oxigen.

Your obliged servant,

A. IBBETSON.

III.

III.

On the Analysis of Sulphate of Barytes. By Mr. JAMES THOMSON. *Communicated by the Author.*

Sulphate of barytes not yet accurately ascertained.

THE analysis of sulphate of barytes has engaged the attention of many distinguished chemists; yet the problem, though of easy solution, may be considered as still unresolved, since the greateſt discordance prevails in their results. The accurate determination of the relative proportions of its constituents, as far as concerns its chemical or mineralogical history, is a matter of secondary consideration; but the soluble combinations of barytes being themselves important inſtruments of analysis, in detecting the presence and ascertaining the quantities of sulphuric acid in any compound by the production of sulphate of barytes; the analysis of sulphate of barytes itself becomes an object of considerable importance, and involves in it the accuracy of the analysis of almoſt all compounds, into which sulphur or sulphuric acid enters.

This an important object.

Its composition according to various authors.

Withering, Black, and Klaproth, who have examined the composition of this salt, agree with Kirwan in stating it as composed of sulphuric acid 33, barytes 67.

According to Fourcroy it is composed of acid 34, barytes 66.

According to Thenard of acid 25·18, barytes 74·82.

According to Berthollet of acid 27, barytes 73.

And according to the experiments of Chenevix, of acid 23·5, barytes 76·5.

Clement and Desormes, in consequence of the discordance of these results, engaged in a series of experiments, which appear to have been conducted with great care; and from which they conclude, that sulphate of barytes is composed of acid 32·18, barytes 67·82.

And Klaproth once more revised and confirmed his former analysis, which gave 33 acid and 67 barytes, as the composition of this salt.

The labours of these distinguished chemiſts, together with the general accordance of their results with those obtained

tained by Richter, Bucholz, Clayfield, and others I have not particularly quoted, might have been supposed decisive of the question; yet in a memoir on the composition of alum, subsequent to that of Clement and Desormes on the barytic salts, and posterior also to the laſt experiments of Klaproth, Messrs. Thenard and Roard have adopted the proportion of 26 per cent of sulphuric acid in sulphate of barytes, as the mean of the results obtained by one of them, and those of Berthollet, after experiments conducted with the greatest care.

The question remaining still therefore undecided, and having myself engaged in a series of experiments on the constitution and properties of the principal mordants employed in dyeing and calico printing, in which I had frequent occasion to ascertain the presence and quantities of various sulphuric salts, I was under the neceſſity of satisfying myself respecting the composition of sulphate of barytes by direct experiments, the particulars of which form the subject of this paper. Occasion of the present paper.

On comparing the results of the different experiments on this subject, it will be seen, that, with the exception of those of Thenard, Berthollet, and Chenevix, they all agree in stating the proportion of acid between 31 and 34 per cent; the mean of the whole, and by far the greater number, making it about 33. Klaproth, Clement and Desormes, and others, have deduced the composition of sulphate of barytes, from that of the carbonate and nitrate; and as this mode appeared to me at once simple and unobjectionable, I followed it in the first instance exactly. Comparative results of former analyses.

Carbonate of Barytes.

One hundred grains of carbonate of barytes were dissolved in dilute muriatic acid, with all the precautions necessary to prevent the dissipation of the solution, or loss from too rapid disengagement of the carbonic acid. When the effervescence ceased, the last portions of gas were expelled by a momentary exposure to heat. The loss amounted to 21·65 grains. The experiment repeated on 50 grains of the carbonate gave 10·85 grains, or 21·7 per cent; Carbonate of barytes dissolved in muriatic acid.

cent; and a third experiment 21·85 grains. The mean of these results gives the proportion of carbonic acid in 100 of carbonate of barytes as 21·75 grains, a quantity which differs only $\frac{1}{4}$ of a grain from that obtained by Klaproth, or Clement and Desormes, who make it 22 per cent.

The solution precipitated by carbonate of ammonia.

2. The muriatic solution, containing 100 grains of carbonate of barytes, was precipitated by carbonate of ammonia. The precipitate, well washed and dried at a heat below ignition, weighed 100·2 grains.

The artificial carbonate similar to the native.

3. One hundred grains of artificial carbonate of barytes, precipitated from very pure muriate of barytes by carbonate of ammonia, and dried at a temperature somewhat below ignition, were redissolved in dilute muriatic acid, and the loss of weight carefully ascertained. The experiment repeated afforded the same result as the preceding with the native carbonate, establishing the identity of the two combinations, and proving, that carbonate of barytes both native and artificial is composed of

Carbonic acid	21·75
Barytes	78·25
	100

Nitrate of Barytes.

Carbonate of barytes dissolved in nitrous acid.

One hundred grains of carbonate of barytes, dissolved in nitrous acid, and gradually evaporated to dryness, afforded 132 grains of nitrate of barytes. The experiment repeated on larger quantities, with a view to the preparation of this salt for the purposes of analysis, gave precisely the same results. One hundred and thirty-two grains of nitrate of barytes therefore contain 78·25 grains of barytes, the quantity contained in 100 of the carbonate; and 100 parts of the nitrate are composed of

Composition of the nitrate.

59·3 barytes,
40·7 acid and water.

100

Clement and Desormes obtained 130 grains of nitrate of barytes only from 100 of the carbonate, which gives for the composition of nitrate of barytes, 60 barytes, 40 acid and water.

water. It is here our experiments chiefly disagree; but the difference does not amount to one per cent, and more perfect accordance will hardly be expected by those, who are in the habit of making such experiments.

Sulphate of Barytes.

One hundred grains of carbonate of barytes were dissolved in muriatic acid, in a platina crucible, and precipitated by sulphuric acid. After slow and careful evaporation to dryness, the crucible was exposed to a white heat during half an hour, and afterwards weighed. The calcined sulphate of barytes amounted to 116·8 grains.

Muriatic solution precipitated by sulphuric acid.

2. One hundred grains of nitrate of barytes were decomposed by solution of sulphate of soda added in excess, and the mixture gently heated. The precipitate well washed, dried, and calcined, weighed 88·6 grains.

Nitrate of barytes decomposed by sulphate of soda.

Now 100 grains of carbonate of barytes contain 78·25 grains of barytes, and produce 116.8 grains of calcined sulphate of barytes;

Composition of the sulphate.

And 100 grains of nitrate of barytes, containing 59·3 grains of barytes, produce 88·6 of sulphate;

From which it follows, that sulphate of barytes is composed of

Sulphuric acid	33·04
Barytes	66·96
	100

The results of the preceding experiments, every one of which was carefully repeated three or four times, and their perfect accordance with those of Withering, Klaproth, and others I have already quoted, left no doubt of their accuracy on my mind.

The cause of the disagreement between eminent chymists sought.

Aware however, that no individual authority, however respectable, can add to or detract from the confidence which the names of Thenard, Berthollet, and Chenevix inspire; and sensible that my single testimony added to the rest would weigh but little in the scale against them; I was desirous, if possible, of detecting the source of this discordance in their experiments, as the surest and only

 means

Thenard's mode of ascertaining the composition imperfectly given.

means of finally deciding the question. In the extract, which Guyton has given of the memoir of Thenard on the different states of antimony, in the 32d volume of the Annales de Chimie, the mode in which he ascertained the composition of sulphate of barytes is not stated with sufficient minuteness, to enable any one to repeat his experiment. One hundred grains of pure barytes, fused in a crucible, are stated to have afforded 133·3 grains of calcined sulphate of barytes; but whether by direct combination, which would be liable to errour, or through the medium of some other solvent, is not mentioned. Nor is the mode by which the pure barytes was obtained noticed in Guyton's extract, though of the utmost importance in this inquiry. The experiment indeed does not appear to have been made in a way favourable to accuracy and precision, though for want of sufficient details it is not possible satisfactorily to point out the sources of errour. The experiments of Berthollet, which determined the proportion of acid in sulphate of barytes at 27 per cent, I am wholly unacquainted with; nor do I know the mode which this celebrated chemist pursued in making them; which I regret the more, as they are stated to have been conducted with scrupulous exactness.

Berthollet's experiments not given.

Mr. Chenevix more particular.

Mr. Chenevix's paper in the Memoirs of the Irish Academy however contains all the details necessary for the examination of his experiments, and fortunately also furnishes additional proofs of the accuracy of my own results.

His process.

To ascertain the quantity of sulphuric acid in sulphate of barytes, Mr. Chenevix decomposed a given weight of sulphate of lime (the composition of which he had ascertained by previous experiments); and having found the quantity of sulphate of barytes, which it afforded, the proportion of sulphuric acid in the latter was readily deduced. "Upon 100 grains of calcined sulphate of lime," says Mr. Chenevix, "I poured some oxalic acid, which attracts the basis with an affinity superior to that exercised by sulphuric acid. Oxalate of lime was here formed, but oxalate of lime is soluble in a very small excess of any acid. A little muriatic acid operated a complete solution, and thus

thus a great quantity of sulphate of lime required but little water to dissolve it. Into the liquor muriate of barytes was poured, and suffered to remain some time gently heated; by these means any oxalate of barytes, that might have been formed, was retained in solution by the original excess of acid, and the entire quantity of sulphate of barytes was deposited. Of the exactness of all those methods, which I used as the instruments by which I ascertained these results, I convinced myself by various preliminary experiments. After the usual filtration, washing, and drying at the gentle heat of a sand bath, I obtained in one experiment 185, in another 183, and lastly in another 180. We may therefore take 183 as the mean proportion. Consequently we shall say, 183 grains of sulphate of barytes contain the same quantity of sulphuric acid, as 100 of sulphate of lime (43); and 183 : 43 :: 100 : 23·5. Therefore 23·5 are the proportion of sulphuric acid in 100 of sulphate of barytes."

This experiment repeated.

I repeated this experiment of Mr. Chenevix with calcined sulphate of lime carefully prepared, and obtained from 100 grains, as he had done, 180·5 grains of sulphate of barytes dried at the heat of a sand bath. Suspecting, however, that the various and complicated affinities, which are brought into play in this process, might be productive of some errour; and that the mode was defective, though the results were correctly given; I dissolved 10 grains of calcined sulphate of lime in a pint of boiling distilled water, and poured in muriatic of barytes. The precipitate, washed, dried, and calcined, weighed 17·7 grains. This accorded so nearly with the experiment of Mr. Chenevix, that I was satisfied of the exactness of his method, and that it was not here I was to look for the source of the discordance. His analysis of sulphate of lime I had not verified, having an indistinct recollection of its agreeing nearly with the composition of this salt as stated by others. On a more attentive examination however I found, that the proportions, as given by Mr. Chevenix *, are the converse of those of Klaproth;

The sulphate of lime dissolved in distilled water.

The proportions of acid and base in the sulphate of lime the cause of the difference.

* Dr. Thompson, in his excellent System of Chemistry, vol. II, p. 355, 2d edition, has, by a very natural mistake in quoting from the Phil.

Klaproth; the former making it contain 57 parts of lime and 43 acid in a hundred, and the latter 57 acid and 43 lime nearly. I at first imagined this was a typographical, or perhaps an arithmetical errour; but this is not the case: 100 parts of pure lime afforded Mr. Chenevix 176 grains of calcined sulphate, which gives the proportions exactly as stated in his memoir. Here then evidently hinges the difference in Mr. Chenevix's analysis of sulphate of barytes compared with mine and others; it remained therefore to ascertain, which of the two analyses of sulphate of lime was to be relied on; that which makes the proportion of acid 43 per cent, or that which makes it amount to 57.

These therefore investigated.

Lime dissolved in muriatic acid and precipitated by sulphuric.

1. I dissolved 100 grains of pure lime, prepared as Mr. Chenevix has directed, in muriatic acid in a platina crucible; and, after precipitating with sulphuric acid, evaporated the mass slowly to dryness. The crucible was then exposed during an hour to a white heat. The calcined sulphate of lime weighed 240 grains.

Carbonate of lime dissolved in acetic acid and precipitated by sulphuric.

2. Fifty grains of pure carbonate of lime were dissolved in acetic acid, and sulphuric acid added in excess. The mass, after slow and careful evaporation to dryness, was exposed to a white heat near an hour, and afforded 67·3 grains of sulphate of lime.

Proportions according to these experiments.

The first experiment, in which 100 grains of pure lime afforded 240 of calcined sulphate, gives for the composition of the latter 58·34 acid, and 41·66 lime. The second, if we admit with Dr. Marcet, that carbonate of lime contains 44 per cent of carbonic acid, gives for the composition of sulphate of lime, acid 59, lime 41, which are exactly the proportions of Kirwan. I feel disposed however to place greater confidence in the first result; the experiment was several times repeated, and I think, if we state the proportions in sulphate of lime as 58 acid and 42 lime, we shall not be far from the truth.

These confirm the analysis of

Now Mr. Chenevix found, that 100 parts of calcined

Phil. Mag. vol. XI, p. 115, the proportions of acid and base, as given by Mr. Chenevix in his analysis of sulphate of lime, and thus restored them to accuracy. . This errour has been copied into a work of very inferior merit, the "Chimie appliquée aux Arts" of Chaptal.

sulphate

sulphate of lime afforded 183 grains of sulphate of barytes dried at the gentle heat of a sand bath; but the sulphate of barytes dried at this heat contains still near 3 per cent of water, which deducted leaves 178·5 grains. If we say therefore, that 178·5 grains of sulphate of barytes contain the same quantity of sulphuric acid as 100 grains of sulphate of lime, and that 100 grains of sulphate of lime contain 58 sulphuric acid; we have for the composition of sulphate of barytes, sulphuric acid 32·5, barytes 67·5; which differs only half a grain per cent from what I have myself obtained. sulphate of barytes before given.

Still farther to confirm the preceding results, I made the following experiments. Into a solution of nitrate of barytes I poured 100 grains of sulphuric acid (the spec. grav. of which I omitted to note). Care was taken to have an excess of nitrate of barytes, and the solution was slowly evaporated down to dryness. The precipitate carefully washed from the remaining nitrate, dried, and calcined, weighed 231 grains. Farther confirmations of it.

An equal weight of the same sulphuric acid was poured into a solution of acetate of lime, in which the latter was in excess. After gradual evaporation to dryness, the acetate of lime was separated by repeated washing with alcohol, and the sulphate of lime dried and calcined. It weighed 133 grains.

Lastly, 100 grains of sulphuric acid were poured into a solution of acetate of lead in excess, and the precipitate carefully separated, washed, and dried. It weighed 296 grains.

From these experiments it appears, that 231 grains of sulphate of barytes, 133 grains of sulphate of lime, and 296 grains of sulphate of lead, contain equal quantities of sulphuric acid; and if in estimating the real quantities of acid they contain, we adopt Klaproth's analysis of sulphate of lead as the standard, to which to refer them, we shall have Results of these experiments.

296 grains of sulphate of lead, containing 78·4 grains of sulphate of acid, or 26·5 per cent;

231 grains of sulphate of barytes, containing 78·4 grains of sulphate of acid, or 33·9 per cent;

133 grains

133 grains of sulphate of lime, containing 78·4 grains of sulphuric acid, or 58·6 per cent.

These results, though not in perfect accordance with those I had previously obtained, I considered as sufficiently exact to establish their general accuracy; and I did not think it necessary to verify them by more careful repetition, in which it is possible these slight differences might have wholly disappeared.

General conclusions.

The experiments detailed in this paper then confirm, with trifling variation, the results already obtained by Withering, Klaproth, Kirwan, Clement and Desormes, and others; and prove,

1. That carbonate of barytes, both native and artificial, is composed of carbonic acid 21·75, barytes 78·25.

2. That nitrate of barytes is composed of acid and water 40·7, barytes 59·3.

3. That calcined sulphate of lime contains sulphuric acid 58, lime 42.

4. And lastly, that calcined sulphate of barytes is composed of sulphuric acid 33, barytes 67.

Church Bridge, near Blackburn.

IV.

Experiments on the Expansion of moist Air raised to the boiling Temperature. In a Letter from JOHN GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR,

Objections to the new doctrine of the constitution of the atmosphere,

PERHAPS you will recollect, that I proposed some time ago in your Journal* various objections to the new doctrine respecting the Constitution of the Atmosphere, and the independent equilibrium of its component gasses. The intention of these objections was to invalidate the hypothesis, by showing its inability to explain natural phenomena; and at the same time to point out certain palpable absurdi-

* Vol. XVI, p. 4.

ties,

ties, which are necessary consequences of this novelty in meteorology. This method of examining the subject led me to use arguments, and to avoid experiments made by myself, as much as possible. The choice was suggested by common prudence; for any person can form a correct judgment of a syllogism; the value of which does not depend on the character of the logician, but on qualities that are apparent, and constitute its intrinsic merits or imperfections. On the contrary when an experiment is described, we have no right to expect the reader will assent to the truth of it, until he is convinced of the experimenter's abilities, and of his candour too; which is very liable to suspicion in the course of a controversy.

supported by arguments preferably.

Experiments in certain cases less convincing.

The preceding reasons determined me at the time to defer the experimental part of the refutation to a future opportunity, in hopes, that some other person would undertake the task; but the silence of both parties has hitherto disappointed this expectation, and it almost obliges me to publish certain experiments in my possession; which in all probability will place the controverted point in a clearer light. If air and water be confined by a pellet of mercury in a glass tube, closed at one end, and the apparatus be afterward raised to the boiling temperature, the new hypothesis maintains, that the vapour of the water will make its way through the pores of the permanent gasses, and counteract the pressure of the atmosphere on the pellet of mercury, thereby leaving the included air at liberty to expand indefinitely. The practical method of showing the truth of this proposition by the manometer never appeared satisfactory to me, in consequence of which I undertook to have the experiments repeated in the following manner.

Reason for recurring to [illegible]e at present.

Exp. 1. Barometer 30·06, a tube one twelfth of an inch in bore, and containing a quantity of water in the sealed end, measured $6\frac{1}{8}$ inches from the surface of the water to the open end. A column of air $\frac{4}{10}$ of an inch in length, or something more than $\frac{1}{20}$ of the open space $6\frac{1}{8}$ inches, was confined in contact with the water in the tube by a column of mercury, $\frac{1}{8}$ of an inch long, the temperature of the instrument being 46°. The open end of the manometer was then fixed into the neck of a narrow bottle by means of a perforated

Exp. 1.

Exp. 1. perforated cork, which was made watertight; and the edge of this end projected about $\frac{1}{2}$ a line above that extremity of the cork which entered the bottle, so that the sealed end of the tube, which was out of the bottle, fell $5\frac{1}{2}$ inches below the neck when the bottom was turned upwards. Things being thus prepared, the bottom of the phial was cut away to open a free communication betwixt the atmosphere and the orifice of the manometer. A strong wire was then tied round the bottle, by which it was kept in an oblique position in a large pan of water, so that the open end of the manometer was 3 inches below the surface. At the same time the interposition of the cork and bottle preserved this aperture dry and exposed to the air. The intention of the preceding arrangement scarcely requires an explanation, for it is evident, that, if the pan were made to boil, the tube would receive all the heat which the water could communicate to it, and the size of the boiling vessel was such, as to permit the manometer to be suspended in it free of the sides and bottom, which is a necessary precaution. Lastly, the oblique position of the tube gave the pellet an opportunity to roll over the edge of the orifice, after which it would remain on the cork, provided the spring of the air proved sufficient to expel it. In order to find if this would really be the case, the pan was gradually heated from 46° to boiling, with the manometer suspended in it: and after the water had continued to boil a few minutes, the instrument was taken out of the pan; upon which the mercury was seen to descend quickly towards the sealed end of the tube. According to this experiment the gas or gasses of the manometer were limited in expansion under the pressure of 30·185 inches of mercury to twenty times their original bulk at most. Now the advocates of the new hypothesis say, that the vapour alone sustained 30·06 of this force, or the barometrical pressure. Consequently the dilated air supported nothing more than the weight of the mercurial stopple, or $\frac{1}{8}$ of an inch of mercury. But air rarified 20 times will sustain more than $1\frac{1}{2}$ inch of mercury, when the barometer stands at 30·06; neglecting the increased elasticity, which was occasioned in the present instance by raising the pan and its contents from 46° to 212°. May not we safely

The barome-

safely conclude then from this experiment, that the barometrical pressure is not counteracted by free vapour, which certainly would be the case, were the hypothesis in question consonant with the operations of nature?

trical pressure not counteracted by free vapour.

After ascertaining the preceding fact, I was desirous to approximate with a greater degree of exactness to the limit of the expansion, if a proper instrument could be procured. I say a proper instrument, because the manometer appears to be objectionable on two accounts. In the first place it would be difficult to graduate a tube of a moderate length so accurately, as to discover the dilatation by it truly to two or three places of figures. In the next place a manometer of this construction may be made to give different results by a little management, which will be evident from the following experiment.

The manometer objectionable.

Exp. 2. A manometer $\frac{1}{10}$ of an inch in diameter was cooled by water to 35°, and the height of the column of air was then marked on the glass. In the next place the tube was suddenly plunged into water of 95°, and the height of the column marked as before. On cooling the instrument again as suddenly to 35°, the air contracted to its former dimensions; after which the temperature was raised a second time to 95° in a very gradual manner. The consequence was, that the column fell short of its former height by nearly $\frac{1}{20}$ of its length. This circumstance determined me, to prefer an æolipile to a manometer, the method of using which will appear in the following paragraph.

Exp. 2.

Æolipile preferable.

Exp. 3. What I have called an æolipile is a copper vessel of a conical figure and having a flat bottom. The slender part of the truncated cone has an aperture $\frac{1}{8}$ of an inch in diameter, which is turned directly downwards when the bottom of the æolipile is parallel to the horizon. 110 grains of water at the temperature of 64° were put into this vessel, which required the addition of 2895 grains of water at the same temperature to fill it. Things being thus prepared, the æolipile was immersed in a large pan, and suspended free of the sides and bottom by wires. The pan was then heated to 212°, and kept boiling for some time; after which it was reduced to 64° as quickly as possible by pouring cold water

Exp. 3.

water into it. The æolipile was then removed from the pan, the aperture being covered by the finger of the operator. After being carefully wiped with dry clothes, it was weighed; and found to contain 185 grain measures of air, which was evidently saturated with moisture, and at the temperature of 64°. But 53 measures of air thus circumstanced contain 52 measures of dry air. Thus it appears, that 181·5 measures of dry air at 64° occupy 2895 such measures when raised to 212° in contact with water of the same temperature: whence it follows, that 1 measure of dry air dilates so as to become equal to 15·95 measures in similar circumstances. It is proper to observe, that the barometer stood at 29·66 at the time; and that the height of the water in the pan, reckoning from the mouth of the æolipile, increased the pressure to 29·90: therefore the true dilation of one measure amounts to 16·70. But one measure of dry air at 64° occupies no more than 0·93344 parts of a measure when cooled to 32°; therefore the whole bulk of one measure of dry air raifed from 32° to 212° in contact with water may be stated at 17·100 measures.

Experiments against the existence of an aqueous atmosphere.

I have made several experiments both with this æolipile and a glass flask on air of 64°, which was raised to temperatures less than 212°, but the results did not correspond to the theorem given in the Manchester Memoirs for the purpose of finding the dilatation of moist gasses confined in the manometer. Does not then the evidence of direct experiments authorize us to say, that the existence of an aqueous atmosphere is not proved? or more properly does not the same evidence show this imperceptible fluid to be not only invisible, but also imaginary?

Attention to minutiæ necessary.

Some of your readers may think the preceding experiments are related too minutely, particularly the first and third; but should an impartial person wish to repeat them, he will be of a different opinion. In fact too much precaution cannot be used to prevent the manometer or æolipile from touching the bottom of the boiler; for if this be not done, the experiment will fail, as I have found on different occasions; and this has happened when the water in the pan did not boil. I should also recommend a wide cylindrical boiler in preference to a small vessel with a long narrow neck

neck; because the resistence which vapour meets with in its escape from the latter will in all probability augment its temperature.

The foregoing remarks are confined to the gas of water, which is supposed by the new hypothesis to exist independently in the atmosphere; but I possess observations and experiments respecting the permanent gasses, and their mutual impenetrability, which want of room obliges me to omit at present.

The author has made experiments on the permanent gasses.

Middleshaw, I remain, &c.
May 22d, 1809. JOHN GOUGH.

V.

An Essay on Manures. By ARTHUR YOUNG, *Esq., F.R.S.*

(Continued from p. 128.)

Paring and Burning.

THESE are mechanical operations; and though nothing is directly added to the soil by them, yet the effects are in many instances very extraordinary, and as such ought to be treated of here. There is no subject in husbandry about which so many misconceptions are afloat, or such misrepresetations hazarded, as on this.

Much misconceived and misrepresented.

1. *The Nature of the Ashes resulting from this Operation.*

Effects of paring and burning.

We shall examine the result of burning
1st. Vegetables.
2d. Earths, including, ... 1. Clay, 2. Loam, 3, Sand, 4. Chalk, 5. Peat:

under one of which heads every soil may be arranged.

These two articles will include all that generally comes within the sphere of paring and burning; for the animal substances in this case are too inconsiderable to demand attention,

Destruction of worms and insects.

tention, although the destruction of *living* animals, as worms and insects, is a main benefit of the work.

Paring and burning, says Mr. Kirwan, reduces the roots of vegetables to coal and ashes, and thus prepares both a stimulant and nutriment for plants.

Ashes.

Lord Dundonald observes, that "it is only from the ashes of fresh or growing vegetables, that saline substances, or alkaline salts, are to be obtained; none can be got from peat or decayed vegetable matter. The saline matter produced in the process consists of vitriolated tartar; the alkali of the burnt vegetables combining with the vitriolic acid, which in different states of combination is contained in most soils. Vitriolated tartar has very powerful effects in promoting vegetation." It promotes, as Mr. Senebier remarks, the decomposition of water. It will hereafter be seen, that hidrogen is a most active food of plants. Whatever, therefore, assists in this decomposition must act a very important part in vegetation.

Mr. Fourcroy thinks, that the ashes of burnt vegetables, which have been supposed to consist of earth or clay, when the fixed alkali is washed from them, are principally calcareous phosphorus, like those of animal bones. Lord Dundonald is of the same opinion. This observation is a most important one, and ought to be pursued. In regard to the calcination of earths, that of clay and chalk has been already treated. The circumstances are numerous in which this operation may be highly beneficial.

Loam or sand.

Loam is composed of various combinations of sand, clay, and calcareous earths. The effect of fire exerted on sand, whether mixed in the form of loam, or by itself in a sandy loam, has not been sufficiently ascertained; and to draw conclusions from theory would be dangerous. If I were to reason upon the point, I should imagine that fire would add nothing to the nature of sand which could render it more fertile. The tendency of its operation would be to lessen its small degree of cohesion, from whatever cause arising, and might so far be prejudicial. Iron brought into combination with pure air lessens the aggregation *.

* Davy.

It

It is however a question demanding the combined efforts of the chemist and the farmer, not *reasoning* but *experimenting*.

Effect of heat.

The effect of heat in this operation is remarkable. Whereever burning has been much practised, experience has demonstrated the necessity of removing all the ashes where the fires were made; and though careful farmers remove some of the uncalcined earth, still these spots manifest a deeper green in the crop, than is observable in any other part of the field. The *general* warmth diffused may probably have a greater effect than is suspected.

2. *The Properties of the Ashes resulting from Paring and Burning.*

Properties of the ashes.

Vegetable ashes imbibe carbonic acid from the atmosphere *. They act in decomposition, and yield three fourths in carbonic acid, and one fourth a little inflammable; and last many years, by reabsorbing in winter the principles they had lost in summer †.

I imagine that the advantage of paring and burning some soils depends on the heat emitted from the burning vegetable fibres uniting oxygen with the clay, which forms more than the half of the slices of turf as they are dug from the ground ‡.

That the ashes produced by paring and burning operate as a very powerful manure, cannot be doubted; since in nine tenths of the trials that have been made through the wide range of so many counties, the crops which followed have been found to be very great indeed, and generally superior to those procured by means of any other manure.

Caution.

It is not the want of this success that has made so many enemies to the practice, but rather the contrary; the crops have been so large, and so often repeated, *because great*, that the soil has been left in a state of exhaustion.

This is a subject that demands the attention of the experimental chemist more than most others in the theory of agriculture. The examinations which have been made on

* Priestley. † Fabbroni. ‡ Darwin.

the

Good effects not fully accounted for.

the ashes of vegetables, and of earths, will account for a certain degree of benefit resulting from their use; but perhaps it does not fully account for the enormous crops, which are gained by the operation of paring and burning. I have gone through not an inconsiderable course of reading, with a view to discover the theory of this fact; but my research has not entirely satisfied me. The formation of charcoal, sulphate of potash, and phosphate of lime, with the decomposition of water, and the oxigenation of clay, added to the mechanical change effected by the fire, may certainly account for a considerable part of the improvement.

3. *The Paring and the Burning.*

Method of performing the operation.

The common practice is to pare from two inches on peat soils to half an inch on others: an inch is the more general depth.

Mr. Wilkes, of Derbyshire, has ploughed nine inches deep, and burnt the whole furrow with the assistance of coal *sleck;* manuring double the quantity of land burnt, but working an immense improvement on the space thus deeply burnt. I have seen other cases in which four inches depth was burnt with great success. In the fens of Cambridgeshire the paring is done with a plough, and the depth from one inch to two. On sand the paring should be as shallow as possible.

The chief attention paid in burning is to guard against too great a calcination; as the general opinion of those who have most practised this husbandry is, that the turfs should be rather scorched or charred than reduced to ashes. If burned during a brisk wind, sands frequently vitrify, and will not afterwards in many years, if ever, be restored to a state capable of contributing any thing to the support of vegetables: hence it is a practice with those who are aware of it, prior to burning, to shake out, in dry weather, from the grass-roots, the greatest part of their substance with harrows. The heaps should always be small, and the fire be applied on the sheltered side of them: this method, in a degree, should be regarded in the burning of earths of almost

most every kind; as hereby alone a carbonized substance, called the black ash, will be obtained; instead of a red brick earth, of much less fertility in the outset, afterwards less susceptible of its principles as imbibed from the atmosphere.

In practice, however, as I have found more than once on my own farm, other circumstances will govern this point; such as, the weather in drying the turf, the depth to which pared, and the age of the grass; for these points have all an influence on the size of the heaps.

4. *State in which the Ashes are applied.*

Here occurs a considerable variation in common practice. There are two methods; one, to spread and plough in immediately; the other, to spread immediately, but to leave them exposed to the atmosphere some months before turning in. Mr. Wedge, on the thin sand soil on a chalk bottom of Newmarket heath, had in one field a treble experiment; part was pared and burnt in the spring, and the ashes spread and exposed till ploughing in the autumn for wheat; part pared and burnt late, the ashes left in heaps, and spread just before ploughing for wheat; the third pared, and not burnt at all, by reason of bad weather. The first was by far the best; the second the next; and the third beyond all comparison inferior. This seems to be a decided proof, that the ashes absorb some matter from the atmosphere, which adds to their fertilizing qualities. Application of the ashes.

5. *Application.*

The circumstances which may with propriety be touched on under this head, are, Mode of applying them.

1st. Spreading.
2d. Depth of tillage.

The fact of the ashes improving more after having been for some time exposed to the atmosphere was probably the motive, which induced Mr. Tuke, of York, to pursue on the wolds of Lincoln a practice that deserves attention. It is

is to pare along the centre of the lands a width sufficient for the heaps and burning; to move the sods, in order to plough the breadth; then to plough it; to make the heaps for burning on the land so ploughed; by which means all the land may be ploughed before the ashes are spread, and by this means kept on the surface: two material objects being attained; 1st, the exposition of the ashes; and, 2d, they are not ploughed to the bottom of the furrow, but kept on the surface to combine with the land, and early sinking prevented.

Evenness of spreading is always a material object, whatever may be the manure.

The universal practice (except in one very singular instance) is to plough the first time very shallow. A multiplicity of observations have convinced the farmers in almost every part of the kingdom, that these ashes have a tendency to sink; and the aim has therefore been to keep them near the surface by shallow tillage, especially at first. The method of ploughing before they are spread entirely obviates the necessity of such a practice.

7. *Season.*

Season of the year.

As the work can only be done in dry weather, it is usually begun in March, in which month the NE. winds are more drying than at any other time. When the space to be burned is large, it is continued till September; and as the ashes are the better for exposition to the atmosphere, any crop may be put in that best suits the farmer's conveniency.

8. *Soil.*

As the *quantity* of the manure thus gained depends entirely on the depth of paring, I pass on to the consideration of soils on which the practice may be recommended.

Soil.

I have tried it myself but on two soils; on mountain peat, and on middling loam: on both these I had entire success. But the information, which the respectable society I address look for, must be derived from more varied experience than

than it is possible for one person to pretend to, I shall therefore select a few cases which will embrace all the soils. These might be multiplied tenfold, but it would swell these papers to too great a length to offer more than a sketch.

Clay.

Mr. Bailey, of Northumberland, speaking from great experience, says, "that he has found this operation the most effectual remedy or preventive of the calamity of the red worm and grubs." The advantage of the practice is the certainty of full crops. "I do not," says he, "recollect an instance where the cultivator was ever disappointed; and it is this amazing fertility, that has tempted many people to go on with repeated corn crops, until the soil was exhausted." Clay.

Loam.

On the enclosure of Stanwell in Middlesex, the allotments succeeded well under the perfect practice of paring and burning; and ill, where the turf was ploughed without the application of fire*. In the former case the land was immediately fit for turnips, tares, barley, and clover. In the latter, the tough wiry bent heath, and dwarf furze, kept the land too light and spungy for any crops; and the farmer will be plagued for many years. The difference between the two methods is more than the value of the freehold in favour of burning. I have observed in various counties the same decided preference†. Loam.

In the enclosure of Enfield Chase, (the soil, loam) Dr. Wilkinson states, from experience, that paring and burning saves a very heavy expense; that the ashes possess most fertilizing qualities; that grasses are thus much sooner to be introduced; thas it is a security againſt the ravages of the worm; and that so far from ruining its staple, the land has afterwards retained its fertility during five successive crops‡.

* And which will be the case 99 times in 100 universally.

† Middleton's Middlesex.

‡ Ibid.

After nine years cultivation of land broken up without burning, it has been noticed, that on being laid down, young furze sprung up generally; burning is therefore absolutely necessary*.

Mr. Exter, near Barnstable, broke up a grass field in an enclosed farm, one half by paring and burning, the other half by fallow. The first crop was wheat; the burnt gave thirty-five bushels per acre, the ploughed seventeen; the former was clean, the latter had much couch. Winter tares; the burnt were fourteen inches long, when the ploughed were only six; when eaten off by sheep, the second growth was in length as twelve to four. The next crop being turnips, and dunged equally, the burnt side was free from the fly. Barley succeeded, which was considerably better on the burnt part. Clover was next, which was closer eaten on the burnt part; and when laid to grass was worth 5s. per acre more than on the ploughed half.

Does not diminish the soil.

Mr. Dalton, of Yorkshire, on a dry loam on limeftone and gravel. "It is a mere chimera to suppose, that the soil is diminished by paring and burning. I have done it in the same field twice in the course of fifteen years, and could not discover it in the smallest degree†." On a light loam in Cornwall, Mr. Ans observes, "I was not *singularly* misled by speculative writers (who, I fear, have much to answer for) to think that burning caused a lasting injury to the earth. I fallowed three fields. I expected them to continue free from moss beyond the common period of its return. I found myself much mistaken; besides the crops failing, like those of some of my neighbours who had not burned, the moss returned as usual. Hence I and all my fellow sufferers from following have totally abandoned this practice, and stick to the ancient one of burning."

"It has been the practice of a friend of mine, and his father before him, and of others before them, for near a century past, (the eftate having been in the family for many generations) on their thin limestone land, constantly to pare and burn after ten years grass. The soil is so thin,

* Middleton's Middlesex.

† Communication to the Board of Agriculture.

that

that the plough scalps the rock; yet no diminution of soil is in the least discovered *."

Sand.

"Upon sand I have tried paring and burning, but unsuccessfully †." But Colonel Vavasour speaks of it favourably on this soil, and from experience. Query, whether this difference of result did not hold to their courses of crops? The former speaks, in another case, of two crops of wheat, and one of oats. The latter, 1. turnips, 2. buck wheat, 3. seeds. If Mr. Wright looked on sand for corn, and not grass, no wonder he was unsuccessful. Sand.

Chalk.

Mr. Boys, near Sandwich, in 1783, pared and burnt twenty acres of loose dry chalk mould, four inches deep, on a hard chalk rock, value 1s. per acre, and sowed barley and sainfoin in March. His whole expense, barley crop included, 53l. Produce sixty-six quarters of barley, at 26s., 86l.: his *profit* 33l., or the fee-simple of the land at twenty-two years purchase, the price at that time. The sainfoin took well ‡. In 1795, he writes to the author of the periodical work just quoted, "Should any of your friends, who so much condemn paring and burning, come into Kent this summer, I can show them several scores of acres of wheat, barley, oats, and sainfoin, now growing on land which has several times undergone the operation:—the crops of sufficient value to purchase the land at more than forty years purchase, at a fairly estimated rent, before the improvement. This will be ocular demonstration to them." Chalk.

Peat.

Twenty years past a field of coarse rushy land was broken up; part pared and burnt, the rest not. Whilst in tillage, Peat.

* Mr. Wright. † Ibid. ‡ Annals.

 the

the part burnt yielded crops uniformly better than the others. It has been down to grass several years; the burnt part is quite free from rushes, and covered with a good sweet herbage; the other part full of rushes, and the herbage coarse*."

Mr. Simpson says, "I ploughed ten acres of moor, on a lime stone bottom, in the part most free from ling, without burning, and I have had sufficient cause to repent it; for I have not had even one middling crop fince; and although laid down with seeds, they have by no means so good an appearance as those sown the same year on similar soils after burning, although I have expended as much lime and manure on this as on any part of the farm †."

Near Orton, on a peat moss, six or eight inches deep, on a stiff bluish clay; the only vegetable produce spongy moss, bent grass, dwarf rush, &c. wet and not drained; pared three inches deep, and burnt in the spring; then manured with thirty bushels of lime an acre; ploughed slightly for turnips, which were not hoed. They were worth 3l. an acre; and being sown with oats, produced seventy bushels per acre ‡."

Miss Graham was the first that pared and burnt moss in Monteith. Several acres, that were burnt above forty years ago, continue to carry a close sward of green grass at this day, without a single pile of heath §.

"Of all the methods of breaking up peaty soils which I have practised or seen, the best mode is paring and burning. I have seen various methods on several thousand acres, but none ever equalled this ||."

(To be continued in our next.)

* North-Riding Report.
† Ibid.
‡ Todd. Society's Transactions.
§ Perth Report.
|| Bailey.

VI.

Table of the Rain, that fell at various Places in the Year 1808, *by the Rev.* J. BLANCHARD, *of Nottingham; with a Meteorological Table for the same Year, by Dr.* CLARKE, *of that Town.*

RAIN TABLE, by the Rev. J. BLANCHARD, of Nottingham.

1808.	Chichester.	London.	Bristol.	Cheltenham.	West Bridgford, near Nottingham.	Horncastle, Lincolnshire.	Chatsworth, Derbyshire.	Manchester.	Ferriby, Kingston upon Hull.	Heath, near Wakefield, Yorkshire.	Lancaster.	Dalton, Lancashire.	Kendal.	Edinburgh.
January	3·04	1·52	1·05	0·80	2·85	0·93	1·39	2·70	0·59	1·14	2·90	3·88	5·25	1·67
February	0·90	1·12	0·53	0·20	2·23	0·77	1·35	1·48	0·92	1·26	2·00	1·85	2·42	2·31
March	1·42	0·20	0·35	0·05	1·30	0·42	0·37	0·24	0·29	0·99	0·00	0·55	0·28	0·65
April	2·67	2·42	5·37	5·05	2·01	3·58	2·57	1·32	2·47	3·49	1·81	1·78	2·80	3·04
May	1·72	1·58	2·99	1·30	2·45	1·65	1·68	1·76	2·01	3·08	2·38	4·14	3·95	1·92
June	1·51	0·78	1·75	5·10	2·20	1·18	3·25	2·05	1·21	2·34	1·25	1·84	2·02	2·61
July	5·67	3·22	2·76	—	1·45	2·50	3·71	2·44	3·24	3·44	4·12	3·91	4·85	2·45
August......	2·69	0·96	3·06	—	1·92	1·69	2·13	2·18	2·44	2·66	3·75	4·87	5·37	7·51
September ...	4·87	4·18	4·36	—	2·45	1·53	3·80	2·71	3·27	3·03	1·23	3·05	2·62	2·50
October	6·41	3·82	5·26	—	1·82	2·77	3·98	5·32	2·99	2·49	7·08	6·53	7·25	2·01
November ...	2·92	2·18	3·08	—	0·80	3·20	2·60	3·10	2·51	3·16	4·27	5·20	3·92	0·74
December ...	2·80	1·00	1·52	—	1·74	4·10	1·98	1·79	5·01	2·91	1·69	2·39	2·61	1·93
Total	36·62	22·98	32·08	12·50	23·22	24·32	28·81	27·09	26·95	29·99	32·48	39·99	43.34	29·34

RECAPITULATION.

	Inches. Decim.
Kendal	43·34
Dalton	39·99
Chichester	36·62
Lancaster	32·48
Bristol	32·08
Heath	29·99
Edinburgh	29·34
Chatsworth	28·81
Manchester	27·09
Ferriby	26·95
Horncastle	24·32
West Bridgford	23·22
London	22·98
Cheltenham, for the first six months	12·50

METEOROLOGICAL TABLE,

By Dr. CLARKE, of Nottingham.

1808.	Thermometer.				Barometer.				Weather.		Winds.				Rain
MONTH.	Highest.	Lowest	Mean.	Greatest Variation in 24 hours.	Highest.	Lowest.	Mean.	Greatest Variation in 24 hours.	Fair.	Wet.	N. & N. E.	E. & S. E.	S. & S. W.	W. & N. W.	Inches. Decimals.
January..	49	17	38·17	19	30·39	28·97	29·79	·85	20	11	2	3	51	37	1·40
February	55	22	38·65	13	30·74	29·42	30·05	·57	21	8	21	3	21	42	·54
March ..	52	32	40·21	6	30·39	29·57	30·12	·31	23	8	65	16	—	12	·58
April	56	30	43·62	10	30·20	29·07	29·79	·51	13	17	14	2	27	47	3·85
May	82	48	59·61	11	30·17	29·47	29·84	·24	21	10	21	20	41	11	1·94
June	72	50	59·95	9	30·25	29·62	29·91	·30	19	11	18	26	19	27	2·32
July	89	54	67·19	12	30·16	29·54	29·89	·21	24	7	19	17	36	21	2·10
August ..	76	53	64·62	10	30·17	29·35	29·78	·71	20	11	7	9	42	35	·91
September	68	40	57·32	9	30·28	29·28	29·76	·38	19	11	29	7	26	28	2·37
October..	60	34	46·31	10	30·32	28·98	29·62	·91	19	12	10	6	40	37	2·57
November	55	30	45·06	13	30·25	28·72	29·76	·68	17	13	26	15	26	23	2·18
December	49	22	37·96	14	30·26	29·08	29·76	·55	21	10	30	4	27	32	1·80

ANNUAL RESULTS.

THERMOMETER.	WIND.
Highest Observation, July 13th,	89°SW.
Lowest Observation, Jan. 22d,	17°SW.
Greatest Variation, in 24 hours, Jan. 22d-23d, ...	19°
The Mean, ...	49·88

BAROMETER.	WIND
Highest Observation, Feb. 25th,	30·74 N.
Lowest Observation, Nov. 18th,	28·72 SE.
Greatest Variation in 24 hours, October 13th-14th, ...	·91
The Mean, ...	29·84

WEATHER.	DAYS.	WINDS.	TIMES.	RAIN.	INCHES.
Fair	237	N. & NE.	262	Greatest Quantity in April,	3·85
Wet	128	E. & SE.	128	Smallest ditto, in February	·54
		S. & SW.	356		
	365	W. & NW. ..	352	Total	22·56
			1098		

REMARKS.

REMARKS.

Situation of Nottingham.

The town of Nottingham is situate in latitude 52° 59′ 35″ north, and in 1° 7′ 0″ longitude west of London. It rises with much grandeur from the banks of the small river *Leen*, gradually increasing its elevation as it extends to the N. E., so that above one half stands on a considerable eminence. The foundation is a soft sand stone rock, easily excavated, and forming excellent cellars. The buildings are chiefly of brick, and commonly three or four stories high. The streets are, in general, narrow. The neighbourhood produces an ample supply of coal, which is the only fuel used in the town. The *Trent*, a fine navigable river, flows, from west to east, within a mile of the town; it is subject to very sudden swells, which sometimes produce floods, that inundate the meadow ground between the river and the town. The atmosphere must be, in some measure, influenced by the evaporation that follows, as well as by the dense haze over the river in summer evenings, and the thick fogs of winter.

Instruments & observations.

The barometer, thermometer, and pluviameter (or rain gauge), are new instruments, made by Jones, of Holborn. The thermometer, on Fahrenheit's scale, is placed outside a window, facing the west, in the centre of the town, but in a situation protected from currents of air, or reflected heat. The observations were made daily, at 8 A. M., 2 P. M., and 11 P. M., and from them the averages are deduced.—The barometer (of the portable kind) is firmly fixed to a standard wall over a stair-case, on a level of 130 feet above the sea. The observations were taken daily at 2 P. M., and from these the mean was obtained.—The pluviameter is placed in a garden, on an elevation of 140 feet above the level of the sea, where it cannot be affected by buildings, or gusts of wind. The observations are taken at the end of each month.—The observations on the wind were made at 8 A. M., 2 P. M., and at dusk, from the vane of a church steeple, the most elevated part in the town.

The following Copy of a Monthly Journal will be the best elucidation of the plan that has been pursued.

METEOROLOGICAL JOURNAL for APRIL, 1808.

Day.	Therm. 8 A.M.	Therm. 2 P.M.	Therm. 11 P.M.	Bar. 2 P.M.	Winds 8 A.M.	Winds 2 P.M.	Winds Dusk.	Weather.
1	35	39	30	29·69f.	NW.	NW.	NW.	Snowy
2	32	12	33	29·84r.	SW.	SW.	W.	Fine
3	35	49	41	29·75f.	SW.	SW.	W.	Snowy, hail & rain
4	42	52	51	29·36f.	SW.	SW.	S.	Windy, showery
5	50	53	44	29·07r.	SW.	W.	SW.	Stormy
6	46	48	51	29·58f.	SW.	E.	SW.	Rain
7	54	56	48	29·76f.	SW.	W.	NW.	Showery
8	44	50	40	29·82f.	NW.	NW.	N.	Showery
9	43	57	44	30·20f.	NW.	W.	W.	Fine
10	46	52	46	30·20s.	W.	W.	W.	Cloudy
11	46	51	46	30·20f.	W.	SW.	W.	Cloudy
12	48	55	46	30·14r.	NW.	NW.	NW.	Showery
13	46	54	46	30·21f.	W.	W.	W.	Fine
14	47	56	45	30·13f.	NW.	W.	SW.	Fine
15	48	55	43	30·04f.	W.	NW.	W.	Cloudy
16	40	48	35	30·07r.	NW.	NW.	N.	Cloudy
17	38	39	37	30·09f.	SW.	NW.	NW.	Hail, rain
18	35	45	37	29·96f.	NW.	NW.	SW.	Cloudy
19	35	43	35	29·52f.	SW.	SW.	SW.	Snowy
20	37	50	35	29·55f.	SW.	SW.	NE.	Cloudy
21	33	38	36	29·23r.	NW.	NW.	W.	Snowy
22	38	43	39	29·11r.	SW.	SW.	SW.	Drizzly
23	38	43	40	29·42s.	NW.	NW.	NW.	Rain
24	40	44	38	29·72r.	N.	NW.	NW.	Showery
25	38	43	39	29·86r.	NW.	NW.	NW.	Showery
26	40	47	41	29·86s.	NW.	SE.	S.	Showery
27	43	45	40	29·95s.	N.	NE.	NE.	Showery
28	43	49	41	29·92r.	N.	NE.	N.	Cloudy
29	41	47	43	29·93s.	N.	N.	NE.	Cloudy
30	44	45	49	29·80f.	SW.	SW.	W.	Fine

Thermometer.		Wind
Highest observation	56°	W.
Lowest ditto	30	W.
Greatest variation in 24 hours	10°.	
Mean in the Morning	41·50	
Afternoon	48·06	
At Night	41·30	
Mean for the Month . .	43·62	

Barometer.		Wind
Highest observation	30·20f.	W.
Lowest ditto......	29·07r.	
Greatest variation in 24 hours	..·51	
Mean for the Month....	29·79	

Rain for the Month 3·85 In.

Weather.	Days
Fair	13
Wet	17
	30

Wind.	Times		Times
N.........	8	S.	2
N. E.	6	S.W.	25
E.........	1	W.......	19
S. E.......	1.	N.W. ...	28
			90

Observations.—April 3d, Much snow fell in the morning, followed by hail and rain. 4th, Heavy rain in the night, with much wind. 5th, The weather tempestuous until evening, and much rain in the night. 7th, Heavy rain in the night. 8th, Sharp frost in the night. 15th, Sharp frost in the night. 16th, Hail storm at 7 P. M. and sharp frost in the night. 17th, Several hail storms in the afternoon, and sharp frost in the night. 20th, Heavy fall of snow in the night. 23d, Much heavy rain in the night.

Nottingham.

VII.

Observations on Sulphuric Ether, and its Preparation; by Mr. Boullay, *Apothecary, of Paris* *.

THE use of sulphuric ether is at present very extensive, and its consumption so great, that it has become a produce of the arts in the large way. Its preparation, though much simplified, still merits attention; and appears capable of being improved, not only in respect to economy, but also as to the purity of the product. The making of sulphuric ether may be improved.

In the formation of sulphuric ether, whether by the distillation of a simple mixture of concentrated sulphuric acid and alcohol, or the addition of fresh alcohol to the residuum, all the quantity obtained is not equally dulcified; and, in spite of careful rectifications, the last portions always retain a more or less unpleasant smell, that may be ascribed to some oil intimately united with it, which it is very difficult to separate completely. The latter products always impure.

According to the theory of Messrs. Fourcroy and Vauquelin, founded on their learned researches into the subject, the attraction of sulphuric acid for water, assisted by heat, determines the transformation of alcohol into ether. This reaction of the principles of alcohol, exerted under the influence of the sulphuric acid, precedes the carbonization of the mixture, the formation of the oleum dulce, the extrication of sulphuric acid, and the other phenomena of the process carried to its end. We may even venture to say, that ether is no longer formed, when these products appear; and that what passes over after that time is only separated the residuum, in which it was contained ready formed. It would be an advantage therefore, to prevent, or at least retard, the appearance of these products, which announce a complete decomposition of the alcohol; and, by adding at a proper time fresh quantites of this liquid, to keep up such proportions, that the etherification may go on much longer. For this it appears necessary, that the sulphuric Theory of the formation of ether. What injurious to it. This should be prevented.

* Annales de Chimie, vol. LXII, p. 242.

acid

acid should never compose more than two thirds of the contents of the retort, and that the alcohol should be scarcely ever less than the other third *. In this way the sulphuric acid is prevented from burning the alcohol to its loss, and we obtain none of the results of a decomposition carried too far, which is injurious to the etherification, and immediately follows it. We shall then have a better product, and in larger quantity; and the production of ether will continue, till the sulphuric acid is so much diluted by the water formed and separated, as to be unable to effect any change in the alcohol.

Apparatus.

The particular kind of funnel, which has facilitated my making ether by means of the phosphoric acid †, and is applicable to many other chemical processes, enabled me to carry this theory into practice in the following manner.

Improved process.

To a large tubulated glass retort, placed on a sand heat, I adapted a glass worm immersed in a vessel of cold water. The extremity of the worm was inserted into the neck of a large bottle, between which and a second bottle filled with water a communication was established by means of a siphon. Into the retort I introduced ten kilogrammes [22lbs. avoird.] of sulphuric acid concentrated to 66°. In the tubulure was inserted the funnel with two cocks, so that its pipe descended nearly to the bottom of the retort, passing through the sulphuric acid. Ten kilogrammes of alcohol at 36° of Beaumé's areometer were then poured in quickly, being conveyed through the acid by means of the funnel. The mixture was very well effected, though with violence; and it was the less coloured in proportion as the introduction of the alcohol was more speedy. The distillation was kept up by means of a fire under the retort; and as soon as about two kilogrammes had passed over, ten kilogrammes

The middle product best.

* The proportions of equal parts of sulphuric acid and spirit of wine, constantly adopted, appear to be most suitable. It is to be observed however, notwithstanding the utmost care taken to separate the alcohol, that comes over first, the product that follows does not attain the lightness, that constitutes true ether, till toward the middle of the process.

† See Journal, vol. XVIII, p. 64, and Pl. II, fig. 4.

of

of fresh alcohol at 40°* were introduced drop by drop, regulating the quantity as nearly as possible by what passed over into the receiver. The process was continued so as to obtain fifteen kilogrammes of a white limpid product, of the most agreeable ethereal smell and taste, containing no traces of sulphurous acid or oleum dulce, and yielding, when rectified on a water-bath, eight kilogrammes of pure ether, with some alcohol of an ethereal smell well adapted for future processes.

The liquid remaining in the retort was of the colour of beer, and very clear. It consisted of nearly the whole of the sulphuric acid employed, some alcohol, water, and no doubt a certain quantity of ether completely formed. The residuum.

This residuum, heated afresh, quickly assumed a black colour, and became sulphurous and oily. In this state it may enter into the composition of Hoffmann's mineral anodyne liquor. The residuum might also be turned to account, by using it as sulphuric acid where the alcohol could do no harm, as for instance, in forming different salts. Purposes to which it may be applied.

VIII.

Investigation of a Problem in the Doctrine of Permutations. By Mr. PETER BARLOW.

To Mr. NICHOLSON.

SIR,

IN the course of a mathematical investigation, in which I was lately engaged, it was necessary for me to determine —How many combinations could be formed out of a given number of things, in which there were several things of one sort, Problem in the doctrine of permutations

* I have observed, that alcohol at 36° is best adapted for the common preparation of sulphuric ether; and that the mixture is less coloured when it is at this strength, than if it contain less water. But at the second addition, as the acid is already weakened, it is better to employ it at 40°.

sort, several things of another sort, &c., by taking one at a time, two at a time, &c., to any given number of things at a time.

has been considered only partially,

I have not been able to find, that this problem has been considered by any authors, at least, that I am acquainted with, who have written on the doctrine of permutations and combinations; except indeed Emerson, and one or two other authors of a later date, who have a similar problem, that is, a partial case of the above general one, which from a repetition of operations would be sufficient for the solution of the present question, but the rule which is given by them for determining the number of combinations in each particular case is so long and tedious, that it is really of no use, being little better, or less trouble, than finding the answer from repeated trials.

and the rule too tedious for practice.

A very simple general rule.

This circumstance led me to consider the problem independently of the measures there adopted, and having fallen upon a very simple rule, which includes the particular case of Emerson's in the general one above mentioned; and as it has not, to the best of my knowledge, been given by any author, who has written on this subject, I have been induced to submit it to you for insertion in your Journal, should you think it deserving a place in that useful work.

Problem.

Problem.

To determine the number of combinations, that can be formed out of a given number of things, in which there are m things of one sort, n things of another sort, p things of another sort, &c.; by taking 1 at a time, 2 at a time, &c., to any given number of things at a time.

Rule.

Rule.

Place in one horizontal row $m + 1$ units, annexing ciphers on the right hand, till the whole number of units and ciphers exceeds the greatest number of things to be taken at a time by unity.

Under each of these terms write the sum of the $n + 1$ left hand terms, including that as one of them, under which the number is placed; and under each of these write the sum of the $p + 1$ left hand terms of the last line. Under each

each of these last the sum of the $q+1$ left terms, and so on, through all the number of different things, and the last line will be the answer: that is, the second term shows the number of combinations taking *one* at a time, the third term, the number of combinations taking *two* at a time, &c.

Example.

Example.

Given a number of the form $a^5\, b^5\, c^4\, d^4\, e^4\, f^3\, g$, to find how many different divisors it has, each of which shall be the product of ten factors, of nine factors, of eight factors, &c.; $a, b, c,$ &c. being prime numbers.

Here $m=5$, $n=5$, $p=4$, $q=4$, $r=4$, $s=3$, $t=1$, therefore by the rules

1	1	1	1	1	1	0	0	0	0	0	$= m+1$ units
1	2	3	4	5	6	5	4	3	2	1	$= n+1$ terms
1	3	6	10	15	20	23	24	23	20	15	$= p+1$ terms
1	4	10	20	35	54	74	92	105	110	105	$= q+1$ terms
1	5	15	35	70	123	193	275	360	435	486	$= r+1$ terms
1	6	21	56	125	243	421	661	951	1263	1556	$= s+1$ terms
1	7	27	77	181	368	664	1082	1612	2214	2819	answers.

That is, the number has seven prime divisions, twenty-seven that are composed of two factors, seventy-seven having three factors, &c.

This rule compared with Emerson's.

I have selected this question, because it includes the particular case given by Emerson in his last example; in order that, by a comparison of both methods, an estimate may be formed of the labour that is saved by this rule. It may not at the same time be amiss to observe, that Emerson has not put down a twentieth part of the work, that is necessary for the operation.

Investigation of the Rule.

Investigation of the rule.

By the developement of the formula $(1+a+a^2\ldots\ldots a^m)\cdot(1+b+b^2\ldots\ldots b^m)\cdot(1+c+c^2\ldots\ldots c^p)\cdot(1+d+d^2\ldots\ldots d^q)$ &c., we shall evidently obtain all the possible combinations that can be formed with $m\,a\,s$, $n\,b\,s$, $p\,c\,s$, $q\,d\,s$, &c.; and, as we proceed in this developement, the law whence the above rule is deduced will be readily perceived.

But,

But, for this purpose it will be best to give determinate values to m, n, p, q, &c.; by which means the operation will be more simple, and at the same time the law of formation will be equally obvious. Therefore suppose $m = 4$, $n = 3$, $p = 2$, then by actual multiplication we have

$$\begin{array}{l}
1 + a + a^2 + a^3 + a^4 \\
1 + b + b^2 + b^3 \\
\hline
1 + \left\{\begin{matrix} a \\ b \end{matrix}\right. + \left\{\begin{matrix} a^2\ 1 \\ a \cdot b \\ 1 \cdot b^2 \end{matrix}\right. + \left\{\begin{matrix} a^3\ 1 \\ a^2\ b \\ a\ b^2 \\ 1\ b^3 \end{matrix}\right. + \left\{\begin{matrix} a^4\ 1 \\ a^3\ b \\ a^2\ b^2 \\ a\ b^3 \end{matrix}\right. + \left\{\begin{matrix} a^4\ b \\ a^3\ b^2 \\ a^2\ b^3 \end{matrix}\right. + \left\{ \&c. \right.
\end{array}$$

And again, multiplying this last product by $1 + c + c^2$, we obtain the following result.

$$1 + \left\{\begin{matrix} a \\ b \end{matrix}\right. + \left\{\begin{matrix} a^2\ 1 \\ a \cdot b \\ 1\ b^2 \end{matrix}\right. + \left\{\begin{matrix} a^3\ 1 \\ a^2\ b \\ a \cdot b^2 \\ 1 \cdot b^3 \end{matrix}\right. + \left\{\begin{matrix} a^4\ 1 \\ a^3\ b \\ a^2\ b^2 \\ a \cdot b^3 \end{matrix}\right. + \left\{\begin{matrix} a^4\ b \\ a^3\ b^2 \\ a^2\ b^3 \end{matrix}\right. + \left\{ \&c. \right.$$

$$c + c\left\{\begin{matrix} a \\ b \end{matrix}\right. + c\left\{\begin{matrix} a^2\ 1 \\ a \cdot b \\ 1 \cdot b^2 \end{matrix}\right. + c\left\{\begin{matrix} a^3\ 1 \\ a^2\ b \\ a\ b^2 \\ 1 \cdot b^3 \end{matrix}\right. + c\left\{\begin{matrix} a^4\ 1 \\ a^3\ b \\ a^2\ b^2 \\ a\ b^3 \end{matrix}\right. + c\left\{ \&c. \right.$$

$$c^2 + c^2\left\{\begin{matrix} a \\ b \end{matrix}\right. + c^2\left\{\begin{matrix} a^2\ 1 \\ a \cdot b \\ 1 \cdot b^2 \end{matrix}\right. + c^2\left\{\begin{matrix} a^3\ 1 \\ a^2\ b \\ a \cdot b^2 \\ 1 \cdot b^3 \end{matrix}\right. + c^2\left\{ \&c. \right.$$

Now, without pursuing the developement any farther, we shall readily perceive, that all the combinations in the second place, in both products, consist of *one* letter, in the third place, of *two* letters, and in the fourth of *three* letters, &c. And farther, that in any term, for example the fifth term, the number of combinations is equal to the number in the fifth, fourth, and third, of the foregoing product; the number of combinations in the fourth term is equal to the number in the fourth, third, and second: that is, the number of combinations in each term is equal to the number in the three last named terms of the foregoing product; and if we had used c^3, then the number in each term would have been equal to the *four* last named terms of the foregoing product; and generally, if we had employed c^p, the number

ber of combinations in each term would have been equal to the number in the $\overline{p+1}$ left-hand terms of the preceding line. And exactly the same law is observed when we multiply this last product by $(1+d+d^2 — d^q)$, that is to say, each term of the new product is equal to the number of combinations in the $q+1$ left hand terms of the line which precedes it; and so on, for any number of multiplications whatever. Whence the truth of the rule is manifest.

We may farther remark, that, if the greatest number of things to be taken at a time exceeds half the number of things given, still, we need not pursue the operation for more than half the given number, as will be evident from a closer inspection of the above formulæ. For it must be readily observed, that, were we to carry the operation of each multiplication to its whole extent, the terms on each product would increase, from the first to the middle terms, and then decrease again in the same manner to the other extremity of the line.

Yours, &c.

PETER BARLOW.

Royal Military Academy, Woolwich.
May 31*st*, 1809.

IX.

Description of a very sensible Hygrometer. By Lieutenant HENRY KATER, *of his Majesty's 12th Regiment* *.

IN the *Mysoor* and *Carnatic* is found a species of grass, which the natives call, in the Canarese language, *oobeena hooloo*, in the Maratta, *guvataa sæ cooslee*, and, in Tamul, *yerudoovaal pilloo* †. It is met with in the greatest abundance, about the month of January, on the hills; but may be procured in almost every part of the country, and is very generally known. An Indian grass

* Abridged from the Asiatic Researches, vol. IX, p. 24.

† It is the *andropogon contortum* of Linnæus, and may be easily distinguished from all others, by the seeds attaching themselves to the clothes of those who walk where it grows.

Accident

has a beard very sensible of moisture.

Accident led me to remark, that the bearded seed of this grass possessed an extreme sensibility of moisture; and being then in want of an *hygrometer*, I constructed one of this material, which, on trial, far exceeded my expectations.

Hygrometer made of it.

A B C D, Pl. VI, fig. 1, represents a piece of wood, about fourteen inches long, three inches broad,, and one inch and two tenths thick. The upper part is cut out, as in the figure, to the depth of two inches, leaving the sides A and B, about three tenths of an inch thick. The wood, thus prepared, is morticed into a square board, which serves as its support.

Fig. 2 is an ivory wheel *, about an inch and two tenths *diameter*, and two tenths of an inch broad at the rim. A semicircular groove is made in the circumference, of such a depth, that the *diameter* of the wheel, taken at the bottom of the groove, is one inch. Through the axis, which projects on one side four tenths of an inch, a hole is made, the size of a common sewing needle; and, on this, as a centre, the wheel should be carefully turned; for, on the truth of the wheel the accuracy and sensibility of the instrument chiefly depend. From the bottom of the groove a small hole is made obliquely through the side of the wheel, to admit a fine thread. All the superfluous ivory should be turned away, that the wheel may be as light as possible.

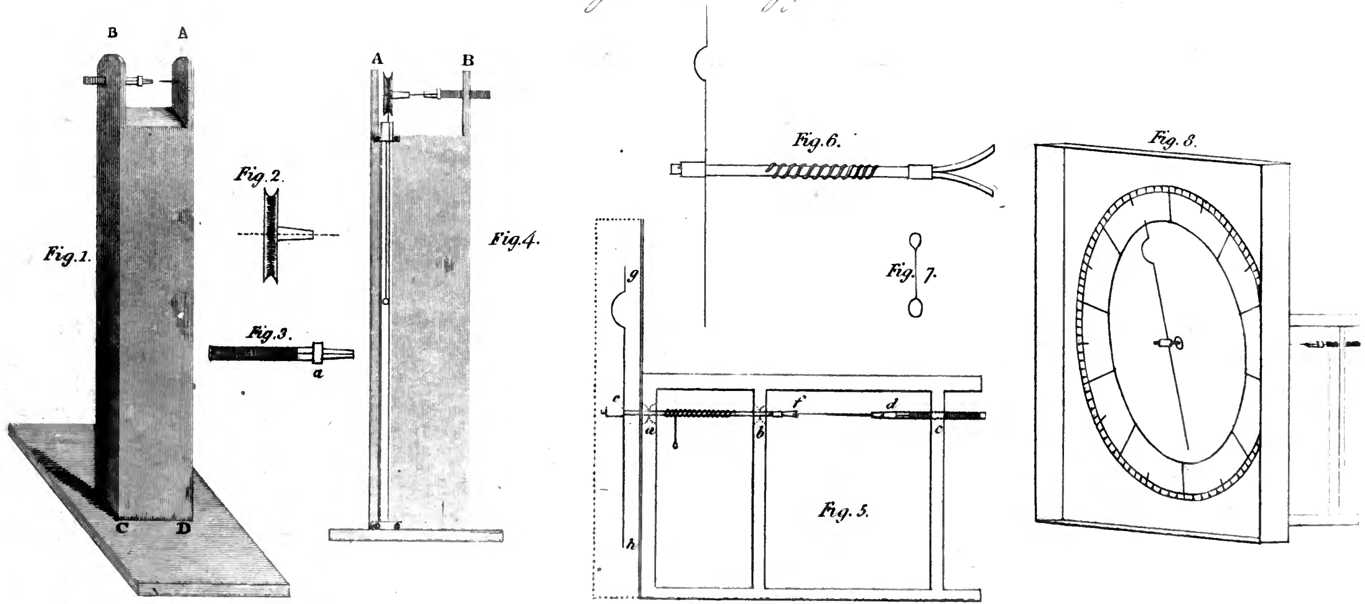
Fig. 3 represents a piece of brass wire, two inches long; on one end of which a screw is made, an inch and a half in length; and, in the other, a notch is cut, with a fine saw, to the depth of half an inch. This part is tapered off, so that the notch, which is intended to hold the beard of grass, in the manner hereafter described, may be closed, by means of a small brass ring (*a*) which slides on the taper part of the wire.

A little below the centres of the semicircles A and B, fig. 1, two holes are made, precisely in the same direction: one of these is intended to receive the screw, fig. 3, and the

* In my first experiments I used a wheel made of card paper, with an axis of wood, which answered very well.

other

Lieut. Henry Kater's Hygrometer.



other a gold pin, which is to project four tenths of an inch beyond the inside of the part A. The pin is made rather smaller than the hole in the axis of the ivory wheel, and is highly polished; in order that the motion of the wheel may be the less impeded by friction.

Two fine threads, about fourteen inches long, are passed together through the hole in the groove of the wheel, and are prevented from returning, by a knot on the outside. To the ends of these threads two weights are attached, *exactly similar*, and just heavy enough to keep the threads extended.

One of the threads having been wound on its circumference, the wheel is to be placed on the pin, about the tenth of an inch from the side A, as in fig. 4. Two glass tubes, of a sufficient bore to admit the free motion of the weights, are fixed in grooves, in such a manner, that each thread should fall exactly in the axis of the tube. The tubes are so long as nearly to touch the ivory wheel.

The beard of the *oobeena hooloo*, being prepared by cutting off that part which is useless, is inserted about the tenth of an inch in the projecting end of the axis of the wheel, and confined by a small wooden pin, which is to be broken off close to the axis; the other end is placed in the notch of the brass screw, before described, and secured by means of the sliding ring.

It is evident, that when the grass untwists, the wheel will turn on the gold pin; and the thread, which is wound about it, with the weight attached, will descend in the one glass tube; while, on the contrary, the weight on the opposite tube will ascend, and *vice versa*. Action of the hygrometer.

The beard of the grass is now to be thoroughly wetted, with a hair pencil and water; and when the wheel is stationary, the weights are to be so adjusted, by turning the brass screw, that the one shall be at the top, and the other at the bottom of the glass tubes; which points will mark *extreme moisture*. Adjustment for moisture

The instrument must then be exposed to the sun, or to some heat, not powerful enough to injure it, but sufficient to obtain a considerable degree of dryness. The weights will now hange situati ; and, probably, on the first trial, and dryness.

will continue to move *beyond* the glass tubes. Should this happen, the beard of grass is to be shortened, by sliding back the ring, and advancing the brass screw, so as to include a longer portion in the notch. Other trials are to be made, and the length of the grass varied, till the extremes of dryness and moisture are within the limits of the glass tubes.

Inconvenience of this form.

If the whole of that part of the *oobeena hooloo*, which possesses the hygroscopic property, be used, the scale will comprise more than *twenty-four* inches; a length, which, though perhaps useful on particular occasions, will not be found convenient for general purposes.

Trial of its accuracy.

From an idea, that in a high state of moisture the grass would not retain sufficient power to move the wheel equably, it was thoroughly wetted, till it indicated extreme moisture, and, while in this state, the wheel was drawn round, by laying hold of one of the threads; on releasing it, it instantly regained its former situation, with considerable force. The same experiment was made, in various other states of moisture, and it was always found, that the weights returned immediately to the degree from which they had been removed.

A metal wheel may be used.

It would perhaps be an improvement, if a light wheel of brass, or any other metal, not liable to rust, were used instead of the ivory one; the grass having been found, by experiment, to be capable of moving a wheel of lead. The axis of the wheel might be made very small, and supported on Ys, which probably would add much to the sensibility of the instrument.

Adapted to slight variation of moisture.

I have yet had no opportunity of comparing this with any other hygrometer; but it is simple in its construction, not easily disordered, and should seem, from the extent of its scale, to be particularly adapted to experiments, in which small variations of moisture are to be observed.

Hygrometrical observations connected with refraction

The *hygrometer* has been hitherto an instrument rather of curiosity than utility. But from most accounts that we have, it appears very probable, that this instrument has more to do with the phenomena of *refraction*, than either *barometer* or *thermometer*. If then we could obtain a number of observations of apparent altitudes together with

with data from which to calculate the true, noting at the same time the *hygrometer*, *barometer*, and *thermometer*, perhaps some law might be discovered, which might enable us to ascertain the quantity of the effect of moisture on refraction. It was with this view the *hygrometer* above described was constructed; but not having yet had an opportunity of obtaining the requisite observations, it is to be hoped they may be made by those, who are in possession of time and instruments equal to the undertaking.

X.

Description of an improved Hygrometer. By Lieutenant HENRY KATER, *of his Majesty's 12th Regiment**.

SINCE I had the honour of laying before the Asiatic Society "a description of a very sensible Hygrometer," I have attended much to the improvement of the instrument, and am induced to think that some farther account of it may not be deemed wholly unacceptable. Improvement of the preceding hygrometer.

The principal objection to the hygrometer described in my former paper arose from the necessity of shortening the beard of the *oubeena hooloo*, in order to reduce the scale to a convenient length; this was to be obviated only by giving the instrument a circular form, and inventing some mode of ascertaining without difficulty the number of revolutions made by the index.

A B C D, (Pl. VI, fig. 5) is a frame, made of small square bars of brass or silver; this frame is soldered to a square plate B E, the edges of which are turned up, as represented by the dotted lines, to secure the *index* from injury: on the face of the plate is engraved a circle (see fig. 8) which is divided into *one hundred* equal parts. Three holes, *a*, *b*, *c*, are made through the frame and plate in the same direction; the holes *a* and *b*, are of a conical form as represented by the dotted lines, and are highly polished to lessen friction; the hole at *c* receives a screw, one end of which is tapered, and has a *notch* cut in it with a fine saw, which may be closed by means of the sliding ring *d*. Decription of the improved instrument.

* Ibid, p. 294.

The

The *axis e f* is made of silver wire, very smooth and straight, and of the size of a large knitting needle; on the axis *a screw* is formed, by twisting a smaller silver wire tightly around it *from left to right:* this screw should be fourteen or fifteen threads in length; the end of the axis, *f*, is divided, and is to be closed by a small sliding ring. As this is the most important part of the hygrometer, fig. 6, represents it on an enlarged scale.

A *loop* and *drop* (fig. 7) is made of fine gold wire, of such a size as that when suspended on the screw it may slide along it with perfect freedom by means of the revolution of the axis, but not escape from one interval to another by any other motion; should the loop, on trial, be found too large (as indeed it ought to be) it may be easily closed a little, by placing it on the screw, and pulling it gently by the drop, it will then assume an elliptical form, as in the figure. This loop is intended to register the number of revolutions made by the index, as it hangs freely from the *axis*, and advances one *interval* between the threads of the screw, for each revolution.

The *index*, *g h*, is made of fine wire, accurately balanced, and as light as possible; it fits on the end of the axis *e*, *and is to be placed at right angles with the commencement of the screw.* (See fig. 6.)

The beard of the *oobeena hooloo* is represented between *f* and *d*, (fig. 5.) The top of it, which is crooked, being cut off, it is first secured between the cheeks of the axis, at *f*, by means of the small sliding ring; the axis is then turned round till the gold loop is brought to the fifth or sixth *interval* of the *screw*, counting from the dial plate; the screw at *c* is then advanced, so as to receive the lower or thick extremity of the beard of the *oobeena hooloo* in the notch, where it is also confined by the sliding ring *d*.

Adjustment of this hygrometer.

The extremes of *dryness* and *moisture* are determined in the following manner. The hygrometer is placed in a new earthen pot, which has never been wetted, and exposed for a considerable time to as great a heat as the grass can bear without injury: when the *index* is perfectly steady, the hygrometer is to be taken out of the vessel, and the screw at *c* turned round with a pair of pincers, so as to bring the gold

gold loop to the *first interval* of the screw on the axis, counting as before from the dial plate, (which is to be placed to the left hand) and the index to 100, or zero. The hygrometer must now be suffered to cool gradually, during which, if the atmosphere be in a mean state of moisture, the index will make four or five revolutions; the *oobeena hooloo* is then to be continually wetted with a hair pencil and water, till the index is again perfectly steady. This will require some time, as it moves very slowly when within a few degrees of *extreme moisture*. The degree at which the index stands is now to be noted, and the number of *intervals* counted between the dial plate and the gold loop, and this number prefixed to the observed degrees will give the extent of the scale.

Reduction of the observations to a standard.

All observations made with this hygrometer are to be reduced to what they would have been had the scale consisted of 1000 parts, or ten revolutions of the index. This is most convenient, as it facilitates the comparison of observations made with different hygrometers. An example may not be thought superfluous. Suppose the scale of the hygrometer to be 1145, or eleven *intervals* and forty-five *parts*; and that at the time of observation, there are *four intervals* between the dial plate and gold loop, and 50 *parts* shown by the *index*; this would be written 450. Then, as 1145 : 1000 : : 450 : 393 nearly, the number of degrees to be registered.

If two of these hygrometers, in which the extremes of dryness and moisture are well determined, be compared together, they will seldom differ *ten divisions* from each other, which is as near a coincidence as can be expected.

The *oobeena hooloo* or *andropogon contortus* is found in every part of the country, in the month of January, when it should be gathered, and thoroughly dried in the sun, before it is used.

Sensibility and other advantages of the instrument.

This grass appears to be far superior to any other hygroscopic substance, hitherto discovered. In the Encyclopædia Britannica, the scale of SAUSSURE's hygrometer is said to consist of 400 degrees, or rather more than *one* revolution of the index; the hygrometer here described makes *eleven* or *twelve* revolutions; it possesses also the advantage

vantage of being perfectly portable, cannot easily be deranged, and may be much reduced in size, if thought necessary, without affecting the extent of the scale.

XI.

On the Germination of Seeds. In a Letter from Mr. J. Acton, *of Ipswich.*

To Mr. NICHOLSON.

Dear Sir,

Physiology difficult of investigation.

It is admitted by the most enlightened philosophers, that scarcely any subject can present itself more difficult of investigation than animal and vegetable physiology. The functions depending on vitality must not be compared to the common chemical processes, or to those changes constantly taking place in nature by the action of inorganic bodies on each other. Life itself is a phenomenon enveloped in mystery, and probably will ever remain so. We can form no judgment of it but from its effects; and those are of so complex a nature, that it is only by the most attentive and studious examination of them we can expect to withdraw the veil of obscurity, under which they are hidden, or at all approximate to the truth. Any suggestions presenting themselves to the mind on so important a subject should be encouraged; and if we can hope to throw the least additional light upon it by our exertions, no obstacles should stop us; not even the (almost) certainty of ultimate failure ought for a moment to lessen the energy of our pursuits.

Functions of organic bodies particularly deserving notice.

Perhaps none of the functions of organic bodies deserve our attention more than those tending immediately to existence, namely the respiration of animals, the germination of seeds, and the consequent vegetation of plants; as also the alterations taking place in the surrounding atmosphere during their operation.

Object of the writer.

The following humble attempts, having for their object the farther illustration of these phenomena by experiment, are with diffidence submitted

through

through the channel of your widely circulated Journal to the eyes of the philosophic world; and if they shall be found of sufficient consequence to clear up any doubt, or induce one single effort in others toward explaining the matters to be treated of; my end will be entirely answered, and my trouble rewarded. They have been undertaken and preserved amidst many interruptions and discouragements; and if they shall be found not to have all the regularity and accuracy to be desired, I trust they will yet have some claim to attention, if not from their originality, at least from the persevering and disinterested industry, which gave rise to them, and brought them to a conclusion, the striving as much as possible to corroborate each experiment by repetition, and the avoiding to make any deductions but such as are fully warranted by facts only.

General opinion, that respiration destroys oxigen, and vegetation restores it.

Since the time of Dr. Priestley, the generally received opinion has been, that in respiration the oxigen gas of the atmospheric air is absorbed, and carbonic acid gas given out; and that in vegetation plants are constantly absorbing the carbonic acid gas as their natural food, and emitting oxigen gas, tending to restore the air to its original purity; in this manner keeping up a regular series of compositions and decompositions, beautiful from their apparent simplicity, and the more deserving of admiration from seeming to harmonize with what was known of the great system of the universe.

This doctrine lately opposed.

No fundamental opposition appears to have been successfully made to this doctrine, till about two years ago; when a work on the subject was published, in which the respectable and learned author* brought together in a small compass almost all the experiments that had been performed, and added a few of his own, for the express purpose of announcing and endeavouring to demonstrate the following theory: "That no air enters the plant or animal during its "appropriate living processes; but that, during the opera- "tion of their respective functions of germination, vegeta- "tion, and respiration, solid carbon is emitted as a secre-

New theory of Mr. Ellis.

* Mr. D. Ellis on Germination, &c.

"tion

"tion in a state of minute division, combines with the oxi-"gen gas of the atmosphere, and forms carbonic acid gas."

His work inconclusive.

I have perused this work with much attention; and, so far from being convinced by it, I can see neither simplicity nor improvement in the suggestions it contains for a new theory: but it appears rather calculated, if the reasoning be conclusive, to throw insurmountable difficulties in the way of satisfactorily explaining or understanding the common functions of respiration and vegetation.

Experiments made to ascertain, whether oxigen gas be absorbed.

Being extremely anxious to ascertain the simple fact of the absorption of oxigen gas, I have for the most part, in conducting my experiments, had this idea constantly in view: I have not therefore turned either to the right or to the left, to quote from or examine those of others, wishing to keep my mind unchecked and unfettered by the reasonings deduced from them, however plausible and respectable they may be.

Advantageous to point out sources of errour.

It is not a very common circumstance, to detail the sources of errour accidentally discovered in a course of experiments; nor is itunlikely, if it were oftener done, but it might prove beneficial in putting others on their guard against the like causes of failure, and prevent much vexation and disappointment.

Instance.

In my own case it has happened, that many experiments and hours of nocturnal labour have been rendered nugatory by the following simple event for some time escaping my notice, and which my previous experience did not lead me to expect. When the subject of this paper first began to engage my attention, I had made some coarse gauze bags exactly suited to the diameter of the mercurial jars I intended to use, that, when filled with the germinating seeds, they might be placed in such a situation as I should prefer in the inverted jars of common air or oxigen, by being thrust up and adhering to the sides. I generally preferred their being near the top, on account of the superior specific gravity of the carbonic acid gas produced, which thus falls down, and makes room for every portion of the oxigen gas to come into contact with the seeds, and be absorbed. The motion of carriages, and other accidental jarring, frequently occasioned the bags to be displaced. To remedy this inconvenience, I took a quill, and, passing the feathered end under the mercury into the jar, returned the bag to its for-

mer

mer situation. After continuing this practice for some time, while engaged in the same manner, I was hastily called away, and left the quill partly in the jar, with one end rising out of the mercury. The jar was then two thirds full of gas, but on my return in about half an hour, I perceived it had increased very considerably, and on placing the quill in other jars, I distinctly heard a shrill whistling noise, like that of air under pressure passing through a capillary tube, and I observed the mercury slowly to sink, till it was on a level on the inside and outside of the jar. I was then convinced the atmospheric air had rushed in by means of the quill, and consequently that all the experiments, in which this had been introduced, must have been vitiated. I reversed the quill, and it still had the same effect. I tried it in jars over water, but no air passed. I afterward made use of string, and other substances, and they all admitted the air through quicksilver, though in different degrees, some being much slower conductors than others. After considering this phenomenon, the best judgment I am able to form of it is, that the air does not pass through the body of the quill, or other substance, but between the mercury and its sides; and in water the passage is prevented by their being in closer contact with each other. Whether this explanation be satisfactory, I leave to your superior knowledge to determine: I confess I was gratified with the discovery, as far as concerned my experiments, as it enabled me to prevent their being so rendered incorrect for the future.

Air of the atmosphere conveyed through mercury by a feather,

but not through water. String, & other substances, acted in the same way.

Mercury does not form a close contact.

It having been stated as a principal argument in favour of the emission of solid carbon from the seed to unite with the oxigen gas of the air, that the quantity of carbonic acid produced was found to be equal to that of the oxigen gas disappearing; upon reflection, it appeared to me replete with difficulty, if not impossible, to ascertain this to any degree of accuracy, from the moistened seeds never ceasing to give out carbonic acid gas, whether oxigen gas be present or not. I was therefore desirous of informing myself upon this subject, and for this purpose I instituted the following method of proceeding.

Argument for the emission of solid carbon in germination questioned.

Exp. 1. Into an inverted jar, containing about 13 cubic inches carefully filled with mercury, I introduced a considerable

Experiments to ascertain the fact.

Germinating barley.

derable portion of barley previously steeped in water, and suffered to germinate till the radicles had shot out about one third of an inch. In passing them under the quicksilver it is almost impossible, even with the utmost care, to avoid the introduction of a small portion of atmospheric air, which closely adheres to them; but this being trifling, the results will not be materially affected by it. The seeds were suffered to remain in this situation from the 11th of February to the 2d of April, the gas being occasionally taken out and tried in the following manner.

Temp. 48°, Pressure 28·68.

In 24 hours····	1·60 cub. in.	63·23	absorbed out of 100 parts by lime water.
same time ··	6·60 ······	91·00	
48 hours····	7·20 ······	98·00	
same time ··	4·90 ······	98·16	
same time ··	6·50 ······	98·18	
3 days······	7·00 ······	98·18	
several days	5·50 ······	98·46	
several days	2·00 ······	99·00	
2d April ····	1·00 ······	99·00	
	42·30	whole of the gas produced.	

A cubic inch was each time exposed to lime water in Pepys's eudiometer. The remaining gas, generally consisting of several cubic inches, was removed into a narrow graduated tube, and a small quantity of a solution of caustic potash passed up. The results of both these trials were compared, and they were as nearly as could be analogous. Two smaller jars were also charged with some barley in the same manner: the gas produced was in proportion to the above, and the absorption nearly similar.

2d Exp. Germinating beans.

Exp. 2. On the 19th of February, temp. 48°, pressure 30·10, eighteen very small beans, freshly germinating, were passed up into aninverted jar full of mercury, holding about 5 cubic inches.

In

In 48 hours....	0·56 cub. in.	90·17	absorbed out of 100.
several days	3·00	98·00	
14 days	3·00	98·47	
several days	2·00	99·00	
several days	2·50	99·00	
	11·06		

Exp. 3. On the 24th of March, temp. 54°, press. 29·34, twenty germinating pease were placed in a similar situation under a jar, containing about $2\frac{1}{2}$ cubic inches.

3d Exp. Germinating pease.

In 3 days	2·00 cubic inches	96·00	absorbed out of 100.
In 3 days	1·00	98·90	
	3·00.		

By these experiments it appears, that seeds, having once begun to germinate, give out carbonic acid gas in considerable quantity, even at low temperatures, though excluded from oxigen gas, and placed in the most awkward and unfavourable situations. And this circumstance should be kept in view, as it will have some influence in determining, if there be a possibility of ascertaining the moment when germination ceases in seeds placed in a confined portion of oxigen gas, or common air, or whether any other carbonic gas be formed, than what is *supposed* to arise from the solid carbon uniting to the oxigen gas, and which has been assumed to be in an equal proportion to the oxigen gas that disappears.

Germinating seeds give out carbonic acid gas, when oxigen gas is excluded.

Now it seems evident, that carbonic acid gas can be readily produced by moistened seeds without the contact of oxigen gas; and in several trials I have observed the gas beginning to appear in a few minutes after passing the seeds up the quicksilver, and when from their being in a healthy vigorous state of germination there was no possibility of incipient putrefaction. In most instances on a small scale, on examination of the gas collected in the first 24 hours, the absorption by limewater has been about 90 per cent: and as this has been an invariable case, even where every precaution was taken for the exclusion of common air, I suspected, that in wholesome germination a small portion of nitrogen gas

A little nitrogen suspected

to be emitted in germination.

gas might be emitted from the seed along with the carbonic acid gas, either by the decomposition of some of its gluten, or by absorbing a small quantity with the oxigen gas of the atmosphere. In the experiment No. 1 it will be seen, that the first tried gas left a considerable residue, owing no doubt to the casual introduction of atmospheric air in passing up the seeds: but the gas, as it formed, being transferred into other jars, this errour, after the first 24 hours, must have ceased to have any effect. Latterly the production of gas became more slow; and if the seeds had been suffered to remain, most likely it would in time have altogether ceased. When they were withdrawn and inspected in the first experiment, no sign of putrefaction appeared; they had an acescent smell, and distilled water poured upon them in a moment deeply reddened paper stained with litmus.

The seeds after the first experiment acescent.

Theory of the production of gas in germination.

Analysis has demonstrated the principal constituent parts of graminaceous or cereal seeds, to be a large proportion of fecula, a little ready formed saccharine matter, and a portion of gluten; which last has been proved to be the active agent in fermentation, and necessary for the conversion of sugar and fecula into alcohol. Therefore, to account for the production of gas in germination, as in seeds placed as in the above experiments, it appears, that, after imbibing a quantity of moisture, the fecula by the action of the gluten becomes gradually decomposed; the already formed saccharine matter is dissolved, and assists in the instant commencement of germination; water most probably is decomposed; its oxigen, uniting to the carbon of the seed, forms the carbonic acid evolved; while the hidrogen in its nascent state, by combining with another portion of carbon, assists the continued conversion of the fecula into saccharine matter; the oxigen gas of the atmosphere is absorbed for the purpose of restoring the equilibrium of the elementary parts, which the decomposition of the matter of the seed, while going on, has a tendency to destroy. But if germination be impeded or stopped, by the exclusion of oxigen gas, or otherwise, the regular composition and decomposition, and consequent changes in the substance of the seed, presently cease: Carbonic acid gas however still continues to be given out, in consequence of the action of the

the gluten on the saccharine matter formed by the germination. When the sugar is exhausted, the acescent first, and then the putrefactive phenomena commence; but only very partially, as I have found the seeds will remain for many months in the jars after the carbonic acid gas has nearly ceased to be produced, without undergoing much apparent alteration.

Exp. 4. To observe how far the same phenomena might take place in matters completely disorganised, and under what variety of circumstances this prolific gas (carbonic) would be produced, I mixed up a little flour, water, and yeast into a stiff paste, and passed a piece of it about the size of a walnut up an inverted jar filled with mercury. In three days I collected seven cubic inches of gas. The whole being submitted to lime water, an absorption ensued, leaving one tenth of an inch only, which appeared to be nitrogen.

Paste gave 6·9 carbonic acid gas, 0·1 nitrogen.

Exp. 5. I also placed in the same situation a piece of paste made with flour and water only, about the same size, rolled very stiff. The gas here formed very slowly, not more than 3·50 cubic inches being collected in ten days. Of this lime water took up 94 per cent. In 8 days, after 4 cubic inches more had formed, and by the same test, 96 per cent were absorbed.

Paste without yeast.

Exp. 6. Three pieces of the same paste were also placed in an inverted jar, containing 1·30 cubic inches of oxigen gas of the purity of 98 per cent. After the paste was in the jar, the whole indicated by the graduated scale 2·75 cubic inches. In three days, the usual allowance being made for difference of temperature and pressure, an absorption had evidently taken place, the volume being reduced to 2 cubic inches. In four days more it increased to 3·70 cubic inches; and in four days after to 7. A little of the air being now tried with limewater, 95 per cent were absorbed; evidently showing, that the greatest part of the oxigen gas had disappeared. To prove this still farther, it was suffered to remain till it had increased to 15 cubic inches, when the same test took up 99 per cent, which it could not have done, had any oxigen gas remained.

Paste in oxigen gas.

Exp. 7. To be convinced no errour had ensued in Exp. 5, I repeated

Exp. 5 repeated.

I repeated it with the utmost care. After some days, 2 cubic inches of gas were collected; and on being submitted to the usual test, 90 per cent disappeared. In ten days after 5 cubic inches more had formed, of which 99 per cent were absorbed; and 3·2 cubic inches being tried with caustic potash, only a bubble remained.

Germination not necessary to the production of carbonic acid.

These results prove beyond any doubt, with how much facility the particles of seeds act upon each other, even in a pulverized state, when moistened with water; and how uncertain, under any circumstances, must be the attempt to discover the precise time of the cessation of germination of seeds confined in oxigen gas, or what part of the carbonic acid gas is given out by that process, and what by the spontaneous decomposition of some portion of the seed. Hence it should seem, that such experiments, as may have been made with a view to establish the identity of quantity between the disappearing oxigen gas and the newly formed carbonic acid gas, must be supposed to be in a great measure fallacious, and consequently the conclusions drawn from them not to be depended upon.

The seeds lost weight

In my first essays on this subject, rendered fruitless by the circumstance before mentioned, I was desirous of discovering whether seeds increased or decreased in weight during germination. For this purpose I weighed accurately several parcels of barley before placing them in the air, and after they were taken out, having previously well dried their surfaces with blotting paper. In every instance I found a deficiency of weight, but not beyond what may be easily accounted for by the evaporation of moisture from the seeds; as I could often, when the air was particularly dry (as oxigen gas prepared from oxigenated muriate of potash over mercury is), perceive some water condensed on the sides of the jars. It appears therefore impossible in this way to come at the truth: but from all I have been able to observe, I am persuaded a real increase takes place. The following statement gives an account of the loss these seeds sustained, while confined for some days in jars of atmospheric air.

by evaporation, not by germination.

200 grains of barley,	lost	8·00 grains
120 grains	lost	6·40
100 grains	lost	5·60
40 grains	lost	2·30
30 grains	lost	2·20
30 grains	lost	2·10

I merely give these results as means of preventing unnecessary trouble and waste of time in others, and not as of any other importance. The seeds were continued in the air, until the increase was considerable, and the oxigen gas was for the most part exhausted, as appeared by the accustomed tests of limewater, and impregnated sulphate of iron.

In proceeding to detail the following experiments, which appear to me decisive of the absorption of oxigen gas, I am compelled to observe, I have found it impossible to vary and continue some of them to the extent I intended, having been often interrupted by the sudden intense coldness of the weather, occasional illness, and the indispensable concerns of business. In most of them, where it was at all necessary, the usual corrections, according to the calculations of Gay Lussac, for change of temperature and pressure were made, and for this purpose the barometer and thermometer at the beginning of every experiment and analysis were duly noted. Experiments decisive of the absorption of oxigen gas.

Sincerely wishing the little experience I may have acquired in this sort of manipulation should be serviceable to others just entering upon the same laudable pursuits, I take the liberty here of strongly recommending the eudiometrical apparatus of Mr. Pepys, as the easiest and most correct that can be used for the analysis of gasses. When accurately made, and the precautions and directions adopted as stated in your Journal, vol. XIX, p. 86, scarcely any obstacle intervenes to prevent its being managed with facility. Great attention should however be paid, when filling the elastic gum bottle with the eudiometric liquor, to the expelling from it every bubble of air; which I have found can be effectually done no other way, than by frequently pressing the bottle in a vertical position, keeping the end of Mr. Pepys's eudiometer. Precautions.

of the bent tube in the liquor the whole time, and suffering it to resume its proper form very slowly. Care should also be taken during the operation, to hold the apparatus firmly at the junction of the tubes with one hand, or cautiously with both; as, when the greater part or the whole of the gas is likely to be absorbed, and it goes on rapidly, the graduated tube will, in consequence of the pressure, sometimes fly off violently from the other, and perhaps be broken. In making the impregnated solution of sulphate of iron with nitrous gas, I dissolve good soft iron in small pieces to saturation, in the purest sulphuric acid I can get, diluted, with about twice its weight of water. The nitric acid is more manageable than the nitrous, and preferable for procuring the nitric oxide to impregnate the iron sulphate, which may be easily done with a wide mouthed bottle in a common basin.

Eudiometric solution.

Caution when the proportion of oxigen in the air is small.

It sometimes happens, when analysing air containing but little oxigen gas, a great deal of nitric oxide is extricated; much more than can be contained in the graduated tube, so that some difficulty arises in attempting to transfer it. In such a case I suffer all the gas to ascend into the elastic bottle, then under mercury take out the tube, fill it with the sulphated solution of iron, replace it, and thus the nitric oxide is again separated, and the experiment completed; care being taken during the time to hold the bottle in such a position, as will prevent the escape of any air.

Experiment with germinating barley in oxigen gas.

Exp. 8. The 14th of March, temp. 40°, press. 29·95.

In these processes it may not be unnecessary to mention, that the jars used were graduated with the nicest accuracy into cubic inches and tenths, by putting into them repeatedly the weight of these measures in grains of quicksilver, and then drawing a line with the diamond. The internal diameter of the largest is not more than 2 inches, and of the others about an inch. A quantity of freshly germinating barley, weighing 760 grs., the radicles protruding about a quarter of an inch, were conveyed in a coarse gauze bag through the mercury into one of these jars inverted, containing 17·20 cubic inches of oxigen gas, prepared from the oxigenated muriate of potash, and of 97 per cent purity; the greatest pains being taken, when the seeds were under

under the mercury, to exclude the atmospheric air from the bag as much as possible, by pressing and turning it round many times. After the seeds were in the jar, the bulk of the whole was increased to 20·11 cubic inches. I had no opportunity of making any observation for some days. On the 21st of March it stood at 19·59, and the next day at 19·98, the difference of temp. and press. being allowed. A part of the air being then conveyed to the eudiometer, and washed with limewater, 87 per cent disappeared, leaving a residue of 13 parts; evidently showing, that the whole of the oxigen gas was not expended. To corroborate this suspicion, I made several trials with the impregnated solution of iron, but owing to the test not being properly prepared, as I found that it acted on the quicksilver, which it should not have done, the results were so anomalous and contradictory, I forbear to state them.

Exp. 9. The 18th of March, temp. 46°. press. 29·80. Eleven germinating beans, weighing 508·3 grs., were passed up a jar containing 6·20 cubic inches of oxigen gas of 99 per cent purity. After the beans were in, the scale indicated 7·65 cubic inches.

Germinating beans in oxigen gas.

In 24 hours it had diminished to 6·80 cubic inches.
In 24 hours more to .... 6·50

On the 24th of March the gas had considerably increased, and upon trial with limewater 88·20 parts in 100 were absorbed. The beans were then taken out, and on being weighed were found to have lost 6·90 grs.

Exp. 10. The 19th of March, temp. 48°, press. 29·72. Twenty germinating pease, weighing 125·5 grs., were placed in an inverted jar, containing 1·60 cubic inches of oxigen gas of 99 per cent purity. When the pease were in, the whole indicated by the scale 1·95 cubic inches.

Germinating pease in oxigen gas.

In 24 hours it had diminished to ..	1·70
In 12 hours more to	1·67
In 24 hours more it had increased to	1·78

On the 24th of March it had increased some inches, and on a portion being examined with limewater, 94 per cent disappeared.

 These

These pease were then passed up a jar *filled with mercury*, and in three days produced 2 cubic inches of gas, 98 per cent of which were absorbed by the same test. Another portion, formed afterward, gave a similar result.

Barley just beginning to germinate in oxigen gas.

Exp. 11. The 19th of March, temp. 48°, press. 29·72. Some freshly germinating barley, weighing 1127 grains, radicles just bursting forth, were placed in a gauze bag, as in Exp. 8, in 24 cubic inches of oxigen gas of 99 per cent purity. When the barley was in, the scale indicated 27·10 cubic inches. In 24 hours it had diminished to 26·70, and in 12 hours more to 26·15. In transferring some of the gas for trial, an accident prevented the farther pursuit of the experiment; but that being exposed to limewater, 34·50 per cent only disappeared.

Germinating pease in oxigen gas, in the dark,

Exp. 12. The 24th of March, temp. 54°, p. 29·34. Some germinating pease, weighing 114·70 grs., were carefully passed up an inverted jar A, covered with brown paper, containing 3·75 cub. in. of oxigen gas quite pure (an inch of it being previously exposed to the test, only a very small bubble remained, hardly appreciable.) When the pease were in, the graduated scale indicated 4·10 cub. in.

In two days in jar A, it had decreased to	3·90
In three days more it had increased to ··	4·20
And the next day to ·················	4·60

The gas being now exposed to lime water, 94 per cent were absorbed; and the pease, on being placed in the balance, had lost some grains in weight as before.

and in the light.

The same weight of pease was placed in jar B exposed to light in 3·77 cub. in. of oxigen gas. After the pease were in, it stood at 41·10 cub. in.

In two days jar B stood at ·············	3·92
In three days it was increased to ······	4·20
And next day to························	4·60

Being now tried with lime water, 93 per cent were absorbed; and the pease, being weighed, had lost two grains only.

Germinating beans in oxigen gas, in the dark,

Exp. 13. The 13th of April, temp. 46°, press. 28·90. In jar A, inverted in mercury, and covered with a wrapper

per of brown paper to exclude the light, containing 2·30 cub. in. of oxigen gas of purity 28 per cent, were placed 6 freshly germinating garden beans. The scale then indicated 3·20 cub. inches.

In jar B, in the same situation, but the light not excluded, the same number of beans were passed up into 2·35 cub. inches of oxigen gas of like purity. The scale then indicated 3·30 cubic inches. and in the light.

In three days it had decreased in jar A, to 2·80
In the same time in jar B, to············ 2·50

The air in jar A being now exposed to lime water, 66·30 per cent were taken up; and of that in jar B 55·50 per cent. The residues being afterward submitted to the impregnated sulphate of iron, the quantity absorbed in each was proportionate to the oxigen gas not consumed, both having about five per cent, which appeared to be nitrogen.

Exp. 14. The 16th of April, temp. 50°, press. 28·90. Germinating beans in oxigen gas in the dark & in the light.

In jar A, covered as before, containing 4·60 cub. in. of pure oxigen gas, 10 germinating garden beans were placed. After they were in, the scale indicated 6·15. In jar B, exposed to light, were also put 9 beans, in 5 cub. in. of the same gas; the scale then indicating 6·85 cub. inches.

In three hours the scale of	jar A indicated	6·06
	of jar B········	6·56
On the 18th of April··	jar A············	5·90
	jar B············	6·30
One the 21st····	jar A had increased to 3	6·10
	jar B to·············	6·80
On the 22d····	jar A to··············	6·40
	jar B to··············	7·20
On the 23rd····	jar A to··············	6·75
	jar B to··············	7·70
On the 24th····	jar A to··············	7·55
	jar B to··············	8·70

6·15 cub. in. being now taken out of jar A, and exposed to solution of caustic potash, 4·75 were absorbed; and of 7 cub. in. out of jar B, the same test took up 5·20 cub. in.

The residue of jar A being submitted to the usual test for oxigen gas, 12.04 out of 100 parts were absorbed: and the residue of jar B being also tried, 18·68 per cent disappeared.

No hidrogen found.

To discover whether any hidrogen gas were present, the portions left were attempted to be inflamed, but not the least sign of it appeared.

The beans were afterward sown, and though the weather proved very unfavourable, some of them continued to vegetate, and are now in blossom.

Nitrogen emitted in germination.

From the quantity of nitrogen left, I am still farther confirmed in the idea, that a little is emitted from the seed in germination, particularly with those of the pulse kind.

Germinating pease in oxigen gas.

Exp. 15. The 16th of April, temp. 50°, press. 28·90.

Fifty germinating pease were placed as above in 2·05 of the same oxigen gas.

The whole then indicated	2·80 cub. in.
In two hours it had decreased to	2·60
On the 18th April, to	2·00
—— 19th it had increased to	2·60
—— 20th	2·80
—— 21st	3·05
—— 22d	3·70
—— 24th	4·60
—— 25th	5·10

4·40 Cub. in. being exposed to caustic potash, only one tenth of a cubic inch remained, which, on being submitted to the test for oxigen gas, was not determined.

Exp. 16. The 19th of May, 1809, temp. 65°, p. 29·50.

Germinating pease in oxigen gas.

Thirty germinating pease, with radicles from half to three quarters of an inch long, were conveyed into an inverted jar containing five cubic inches of oxigen gas of the purity of 98 per cent.

After

After the pease were in, it stood at ...... 6·70
In four hours it had decreased to .... 6·00
In four hours more to .... 5·50
In three hours more to .... 5·10
And in twelve hours more it had increased to 7·00

Six cubic inches of the air being now transferred for examination, 95 per cent were absorbed. The residue tested with impregnated sulphate of iron remained unaltered. Oxigen gas always absorbed at the commencement,

From experiment 8 to this last it appears evident, that, when germinating seeds are first placed in oxigen gas, a considerable absorption takes place, the quantity of which is much influenced by the state of the seeds, and the temperature of the atmosphere. As all I wish to establish is this simple fact, I have not been anxious as to the minor particulars, or in entering into any tedious aud unnecessary calculations, only in instances where the difference of temperature and pressure made it unavoidable; and in such the proper allowances were made, as I have before stated.

In the last experiment it is most decisive, and to an extent beyond any thing to be accounted for by the condensation supposed to ensue from the conversion of oxigen gas and carbon into carbonic acid gas. not to be accounted for by forming carbonic acid.

It is also demonstrated, that, if the seeds be suffered to remain sufficiently long, the whole of the oxigen gas disappears, and the carbonic acid gas notwithstanding still continues to be produced. But if the air be examined when arrived at the original quantity, after the decrease, a portion of the oxigen gas may still be discovered, contradicting at once the statement of the sameness in quantity of the carbonic acid gas formed, and the oxigen gas consumed. The whole of the oxigen disappears, not to form carbonic acid.

In conducting experiments 12, 13, and 14, I thought it might not be superfluous to institute a comparison between the process of germination in the dark and in the light, all other circumstances being as nearly as possible the same: and from an attentive examination and consideration of the results I cannot find any material difference, but what may be readily accounted for by the difference of moisture in the seeds, or some other unknown trifling incident. Here the water was confined to the seeds; but when they are exposed in Action of light unimportant. Evaporation soon kills seeds.

in the open air, and in dry weather, the evaporation from them is rapid, they soon become corrugated, all vital action ceases, and they consequently die. In this manner only can the difference be satisfactorily accounted for; as it is self-evident that the evaporation must be quicker in the light than the shade, the temperature on account of reflected heat being generally much higher; and I have often seen barley seeds vegetate to a considerable height in the dark, when, if they had been thrown to the light, they would have been soon parched up.

Water shows the formation of carbonic acid, and absorption of oxigen.

By the results of the above experiments I am well aware, that, if the seeds, be suffered to remain long enough in the oxigen gas, it at length is all absorbed. This is also easily shown by placing the jars containing the seeds over water: the carbonic acid gas is then gradually taken up by the water, which ascends in the jar, till no more oxigen gas remains. I have sometimes placed large quantities of germinating barley in narrow jars containing from one to three gallons of atmospheric air, and suffered them to remain over water many months. When the remaining air has been tried with the test for oxigen gas, none has been found, nor any trace of any other gas than nitrogen; and this method may be adopted for procuring this gas for experimental purposes, when not wanted in a hurry. It is certainly too a better eudiometrical way of ascertaining the quantity of oxigen gas in atmospheric air, than that of absorption by water sometime since suggested.

Absorption of oxigen.

In referring particularly to experiment 15, it will be seen, that the absorption of oxigen gas in eleven hours was 1·60 cub. in., being nearly one third of the whole quantity employed. This evidence appears to be irresistible, and is beyond what I could have reasonably expected. I have already made a few trials to the same purpose in vegetation and respiration, and hitherto with similar results, which as soon as concluded I shall take the liberty of laying before you. I shall at the same time make some remarks on fermentation.

I remain,

Dear Sir, yours &c.

J. ACTON.

XII.

XII.

Analysis of the Kaneelstein; by Professor LAMPADIUS *.

THE kaneelstein has always been considered as a species of jacinth. Its colour is orange, approaching that of cinnamon, whence Werner gave it this name. Its analysis by Prof. Lampadius leaves no doubt, that it is a variety of the jacinth. He obtained from it Analysis of kaneelstein.

Silex	42·8
Zircon	28·8
Alumine	8·6
Potash	6·0
Lime	3·8
Oxide of iron	3·0
Loss by calcination	2·6
Loss	4·4
	100·0

This analysis shows, that it does not contain much more than one fourth of zircon, while the jacinth contains 0·69.

XIII.

Observation of a Lunar Rainbow; by L. CORDIER, *Mine Engineer* †.

I Was lately witness of a pretty rare phenomenon, a rainbow in the night. The 13th of this month, August 1807, I was standing with several persons on an eminence, that commanded a view of the horizon. We had near us, to the north, the tail of a storm, that poured down a copious rain. At the same time the sky cleared up toward the south, and the moon, nearly at full, appeared. A fine luminous bow then appeared on the storm; but, though it was well defined, the seven primary colours were scarcely to be distinguished in it. They seemed as if drowned in a tint of pale yellow. What struck us particularly was, that the whole of the circle encompassed by the bow was luminous, and tinged with a similar yellow hue, though less intense. Lunar rainbow.

* Journal de Physique, vol. LXV, p. 82.

† Ibid, p. 208.

XIV.

XIV.

On the Want of Tables of the Proportions of the constituent Principles of Salts, and on the Luminous Smoke from Lead Smelting-Houses. In a Letter from a Correspondent.

To Mr. NICHOLSON.

Sir,

Tables of the proportions of the constituent parts of salts would be highly useful.

THERE are few tables more useful to a chemical inquirer, than such as point out the proportions of the constituent parts of salts: not only the philosophic but the practical chemist also would be equally benefited, by having a collection of tables of this description to refer to; and it is I think a matter of surprise, that no person has attempted to publish such upon a scale sufficiently extensive, to answer the purpose of general reference. I was in hopes, that the last edition of your Dictionary would have contained, among its other valuable additions, tables of this kind*; and it may not perhaps be improper to suggest, that this omission may in some measure be supplied by inserting from time to time in your interesting journal, as opportunity of collecting the requisite materials may afford, an alphabetical list of salts, with the proportion of their ingredients agreeably to the latest researches. Such an addition, while it would render an essential service to many of your readers, would not a little increase the value of your Journal.

Luminous smoke from smelting lead ore.

I have observed, that the white smoke that arises from a lead furnace during the process of smelting the ore continues luminous at night for a great length of time after it has left the chimney: sometimes I have seen the smoke retain this luminous appearance until it has been quite dissipated. Your explanation of this phenomenon will oblige, Sir,

Your most humble Servant,

May 6th, 1809. J. S. K.

* In table II at the end of the Dictionary, that of Compounds consisting in general of more than two Principles, the proportions, where they had been ascertained with any accuracy, were given from the best authorities.

I am

I am inclined to think, that the luminous smoke arises from sulphur driven up in the first state of combustion. For sulphur, like phosphorus, may be burned with two kinds of flame, the first not visible in day-light, at less than 300°, as I conjecture, and not capable of setting fire to the smallest thread or vegetable fibre, and the latter much brighter, and generally known. From sulphur in its first state of combustion.

W. N.

SCIENTIFIC NEWS.

THE Russian minister for the home department has communicated to the Imperial Academy of Petersburg the following account of a meteoric stone, weighing about 160 lbs, that fell in the circle of Ichnow, in the government of Smolensko. Meteoric stone in Russia, 13 March, 1807.

In the afternoon of the 13th of March, 1807, a very violent clap of thunder was heard in that district. Two peasants in the village of Timochim, being in the fields at the time, say, that at the instant of this tremendous report they saw a large black stone fall about forty paces from them. They were stunned for a few minutes, but, as soon as they recovered themselves, ran toward the place where the stone fell. They could not discover it however, it had penetrated so deep into the snow. On their report several persons went to the spot, and got out the stone, which was above two feet beneath the surface of the snow. It was of an oblong shape, blackish like cast iron, very smooth on all parts, and on one side resembling a coffin. On its flat surfaces were very fine radii resembling brass wire. Its fracture was of an ashen gray. Being conveyed to the gymnasium of Smolensko, a professor of natural philosophy there considered it at once as ferruginous, from the simple observation of its being extremely friable, and staining the fingers. The particles of which

which it is composed contain a great deal of lime, and of sulphuric acid.

Several meteoric stones in Italy, 19th April, 1808.

On the 19th of April, 1808, at one o'clock in the after noon, a great quantity of meteorolites fell in the commune of Pieve di Casignano, in the department of Taro (formerly the duchies of Parma and Placentia). The air was calm, and the sky serene, but with a few clouds. Two loud explosions were heard, followed by several less violent, after which several stones fell. A farmer, who was in the fields, saw one fall about fifty paces from him, and bury itself in the ground. It was burning hot. A fragment of one of these stones is deposited in the museum at Paris.

Peculiar claw in the beaver.

On the 17th of November, 1807, during an inundation of the Rhone, a beaver was killed in the island of la Barthalasse, opposite Avignon. Mr. Costaing has given a very particular description of the animal, and among other things remarks, that the fourth toe of each hind paw has a double nail, the parts of which close on each other, so as to form a sharp and cutting beak, opening and shutting like that of a bird of prey.

Bees poisoned by the effluvia of the rhus vernix.

A large swarm of bees, having settled on a branch of the poison ash, *rhus vernix*, in the county of West Chester in America, was taken into a hive of fir at three o'clock in the afternoon, and removed to the place where it was to remain at nine. About five the next morning the bees were found dead, swelled to double their natural size, and black, except a few, which appeared torpid and feeble, and soon died on exposure to the air.

Cotton tree introduced into France.

The cultivation of the cotton tree, as well as of the sweet potato from St. Domingo, has been introduced in the southern departments of France, it is said very successfully.

Paper from mountain flax.

Mrs. Lena Serpenti, of Como, to whom an honorary medal was decreed in 1806 for having improved the method of spinning amianthus, has fabricated paper from this fossil, that answers well either for writing or printing, and is capable of resisting the action of fire or water.

Metallic thermometer.

Mr. Urban Joergensen has presented to the Copenhagen Society of Rural Economy a metallic thermometer of his invention, in the shape of a watch. The scale, on a circle on the

the dial-plate, is graduated to 80° of heat and 40° of cold; and the temperature is pointed out by a hand from the centre.

Mr. Creve of Wisbaden has discovered a method of recovering wine that has turned sour. For this purpose he employs powdered charcoal. The inhabitants of the banks of the Rhine have bestowed on him a medal, as a reward for this discovery. Sour wine sweetened by coarcoal.

Mr. Ljung, a Swedish naturalist, has discovered a new species of mouse, which he has named *sorex caniculatus*. It is the smallest animal known of the mammiferous class, weighing only about half a drachm. Diminutive quadruped.

Mr. Lacepède has lately glven a minute description of an oviparous quadruped, not hitherto noticedby any naturalist, but preserved in the Museum of Natural History. He classes it in the genus proteus, or that of salamander, distinguishing it by the name of tetradactylus from the number of its toes. New quadruped.

A German chemist is said to have discovered another new metal among the grains of platina, to which he gives the name of *vestium*. New metal.

Counsellor Koehler, of Moscow, is busily employed in cleaning the old coins he is continually receiving from the Crimea. He is publishing a collection of more than 600 kings or cities, all belonging to Grecian colonies, or kingdoms, that extended along the northern and western coasts of the Black Sea. Ancient coins.

The University of Leipsic has resolved, that the stars belonging to the belt and sword of Orion, as well as the intermediate stars, which have yet received no particular name, shall in future be called the Stars of Napoleon, or the Constellation Napoleon. New constellation.

A Voyage of Discovery to the Countries of the South, by Order of his Majesty the Emperor Napoleon, in the sloops Geographe and Naturaliste, and schooner Casuarina, during the years 1800—1804, compiled by M. F. Péron, Naturalist to the Expedition, is published conformably to a Decree of the Emperor, in 2 vols. 4to, with 41 plates, 28 of them coloured, and 3 large maps. In this work are described the least known parts of van Diemen's Land, the large strait that Voyage of discovery.

that separates it from New Holland, the discovery of the Great Land of Napoleon, the Great Archipelago of Bonaparte, &c.

Index to Buffon. Prof. Sue has published an Analytical and Systematic Index to Sonnini's new edition of Buffon, with an index to the names of authors. The index occupies 3 vols. 8vo., and was highly necessary to a work in 124 vols.

Index Mem. of French Academy. Mr. Demours published an Index to the Memoirs of the French Academy of Sciences in 9 vols. 4to, each volume including ten years of the Memoirs. Mr. Cotte is now employed on a tenth volume, which will make the index complete from the commencement to the year 1790, with which these Memoirs finished.

Capillary pen. A Mr. Baradelle has constructed a pen, which he terms capillary, capable of tracing 144 lines in the space of a French inch.

Dublin Society.

Dublin Society. AT a meeting of this Society, at their house in Hawkins Street, on the 11th of May, various resolutions were passed.

American fir to be compared with that of Europe. It having been suggested to the Society, that the timber imported from North America differs very materially in quality and strength from the timber, which has for many years past been used in this kingdom: it was resolved,

That a committe be appointed to inquire into the truth of the above suggestion; and to report to the Society on the comparative strength of Norway and Memel timber, with that of the timber of North America, in which the committee will distinguish the particular states of North America, whence the timber may have been imported, the comparative qualities of which with those of Memel and Norway shall be reported upon.

Mr. J. L. Foster presented the following report from the committee of chemistry:

Catalogues of Irish minerals. The committee of chemistry and mineralogy, to whom it was referred to report upon Mr. Higgins's manuscript catalogues of Irish minerals, have proceeded to take the same into consideration, and are decidedly of opinion, that it will not

not be expedient to incur the expense of printing any catalogues, until the collections themselves shall have been rendered much more perfect than they are at present. They are farther of opinion, that the nature of these collections requires, that the catalogues should be arranged according to the topographical situation of the specimens, rather than by a systematic distribution into classes; but adverting to the great labour, which would attend the making a catalogue on so opposite a principle from that which has been adopted, they merely recommended for the present, that the professor of chemistry and mineralogy be directed to add a topographical index to the catalogue of each country, specifying under the names of the different places, that are mentioned in the catalogue, the numbers of the various specimens, which have been brought from it.

Geological and mineralogical survey.

The committee have farther taken into their consideration the resolution of the Society of the 7th day of July last, authorising this committee to offer a premium not exceeding two hundred pounds for the best geological and mineralogical survey of the county of Dublin, to be approved of by them, and sanctioned by the board. The committee find, that no person has become a candidate for executing the task that has been thus proposed; and they recommend, that the proposal itself be discontinued. The committee are further of opinion, that the division into counties is, in many instances, an inconvenient mode of assigning a district proposed for mineralogical survey; and they should recommend in preference an attention to the great lines of geological character, which have been traced out by nature. Of these they know of none more interesting than that which marks the coal district in the vicinity of Kilkenny, comprising some portion of each of the three countries of Carlow, Kilkenny, and the Queens-county.

Coal district of Kilkenny.

The committee are of opinion, that no measure would conduce more eminently to the advancement of the agriculture, manufactures, and general commerce of this country, than a complete and scientific survey of its mineral productions; but such a survey as the committee allude to would require a degree of geological science and practical knowledge, such as is possessed by very few, and, if extend-

ed

ed to the whole of Ireland, would demand an expense far beyond the means of the Society.

Still, however, they think it an object well worthy of adoption, to make a beginning, to choose some limited district; to give it in charge to some person of undisputed science, to request from him a map on a large scale, drawn with a view to represent the mineralogical characters of the district, accompanied with sections of the strata, particularly in the vicinity of mines, elucidated by a copious memoir, and accompanied with collections of specimens of the principal substances referred to. If such a beginning were once obtained, printed, and circulated by the Society, it might serve as a useful pattern for farther undertakings; and if executed with that degree of science, which the committee flatter themselves with being able to obtain, might possibly appear of such national importance, as to obtain for the Society more ample funds for its further prosecution.

The committee have thought it their duty to consider which of their present funds are more particularly applicable for the purpose, and in the first place they propose the application of the £200, which had been appropriated to the execution of the survey, which they have already recommended should be relinquished. A more ample fund seems to be available in a part of the £1300 reserved in the estimate toward the completing the statistical surveys of the thirteen counties which remain to be undertaken. Of these they understand but two or three are in any forwardness; and unless the execution of the remainder should be very superior to that of many of those which have already been obtained, the committee are of opinion, that such a survey as they now propose would be an application of the funds of the Society incalculably more beneficial.

County surveys.

In selecting a person for the undertaking, the choice is necessarily confined among very few. Your committee are of opinion, that Mr. Richard Griffith Jun. is eminently qualified for the undertaking; and to him (subject to the approbation of the Society) they have proposed to undertake it. Your committee could have wished to make an arrangement with Mr. Griffith with respect to the amount of remuneration, which he should finally receive; but on suggesting

ing to him, (subject to the approbation of the Society) to accept of the £200 above alluded to, as soon as his map and memoir should be executed and approved of. Mr. Griffith on one hand setting a higher value on his time as a professional man, than the Society could at present afford to give, but on the other hand not desiring to make this undertaking an object of emolument, prefers to submit to the Society, at the completion of the work, an account of the mere expenses incurred in its prosecution, proposing that the Society should discharge the amount on accepting of his work; and your committee, considering the great liberality of the proceeding on the part of Mr. Griffith, and the great advantages which may be expected from its execution, earnestly recommend it to the adoption of the Society.

The survey undertaken by Mr. R. Griffith.

Mr. Leslie Foster, who made the report, stated, that he conceived the committee were fully justified in selecting Mr. Griffith for this undertaking, as he had heard the late Mr. Greville, one of the first mineralogists in Europe, who was a patron of the Geological Society in London, and Vice-president of the Royal Society, declare, that Mr. Griffith's professional acquirements, as a mineralogical engineer, rendered him fitter than any other man in Great Britain or Ireland, that he was acquainted with, to make a mineralogical survey; as such an undertaking required an intimate knowledge of the most approved methods of working mines, as well as of the sciences of geology and mineralogy.

This report of the committee was adopted accordingly, and confirmed by the society at large.

Mr. Davy.

Mr. Davy intends to visit Dublin next winter, and give a course of electro-chmeical lectures on his late discoveries.

ERRATA.

Page.	Line.	
139,	24,	*For* p. 133, *read* pages 133 and 158.
158,	18	—— of —— by.
179,	28	—— muriatic—— muriate.
180,		note *before* the proportions *add* reversed.
181,		lines 1 & 3 from bot. *for* sulphate of *read* sulphuric.

METEOROLOGICAL JOURNAL

For JUNE, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

MAY Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
27	56	58	62	57	29·60	Fair	Fair
28	58	57	61	53	29·63	Rain	Ditto
29	57	58	65	47	29 56	Ditto	Ditto
30	51	51	61	53	29·82	Ditto	Cloudy
31	49	56	60	51	29·78	Rain	Ditto
JUNE							
1	60	58	72	50	29·50	Fair	Fair
2	51	51	58	50	29·36	Rain *	Cloudy
3	51	56	62	46	30·02	Fair	Fair
4	58	57	71	53	29·68	Rain	Ditto
5	57	53	62	51	29·33	Ditto	Rain †
6	56	55	60	50	29·61	Ditto	Fair
7	54	55	60	50	29·73	Ditto	Ditto
8	55	56	60	51	29·78	Ditto	Cloudy
9	55	56	60	51	29·55	Ditto	Ditto
10	55	56	60	49	29·57	Ditto	Ditto
11	54	55	57	50	29·73	Ditto	Ditto
12	58	56	61	50	30·08	Fair	Ditto
13	58	57	62	55	30·00	Ditto	Fair
14	59	58	67	56	29·97	Ditto	Ditto
15	58	58	64	52	29·88	Ditto	Ditto
16	59	58	66	54	29·95	Ditto	Ditto
17	58	59	69	54	29·82	Ditto	Ditto
18	58	56	63	49	29·82	Ditto	Ditto
19	58	62	67	57	29·90	Ditto	Ditto ‡
20	62	67	73	58	30·02	Ditto	Ditto §
21	62	65	72	59	30.22	Ditto	Cloudy
22	63	63	66	57	30·30	Rain ‖	Fair
23	64	64	73	58	30·31	Fair	Ditto
24	64	64	72	58	30·33	Ditto	Ditto
25	61	58	68	50	30·43	Ditto	Ditto ¶

* Rain, boisterous, and cold, all the forenoon.
† Evening boisterous and very cold.
‡ The Moon obscured at intervals; at 12 dark and windy.
§ Afternoon sultry; in the evening refreshing breezes.
‖ Heat drops.
¶ Great change to cold.

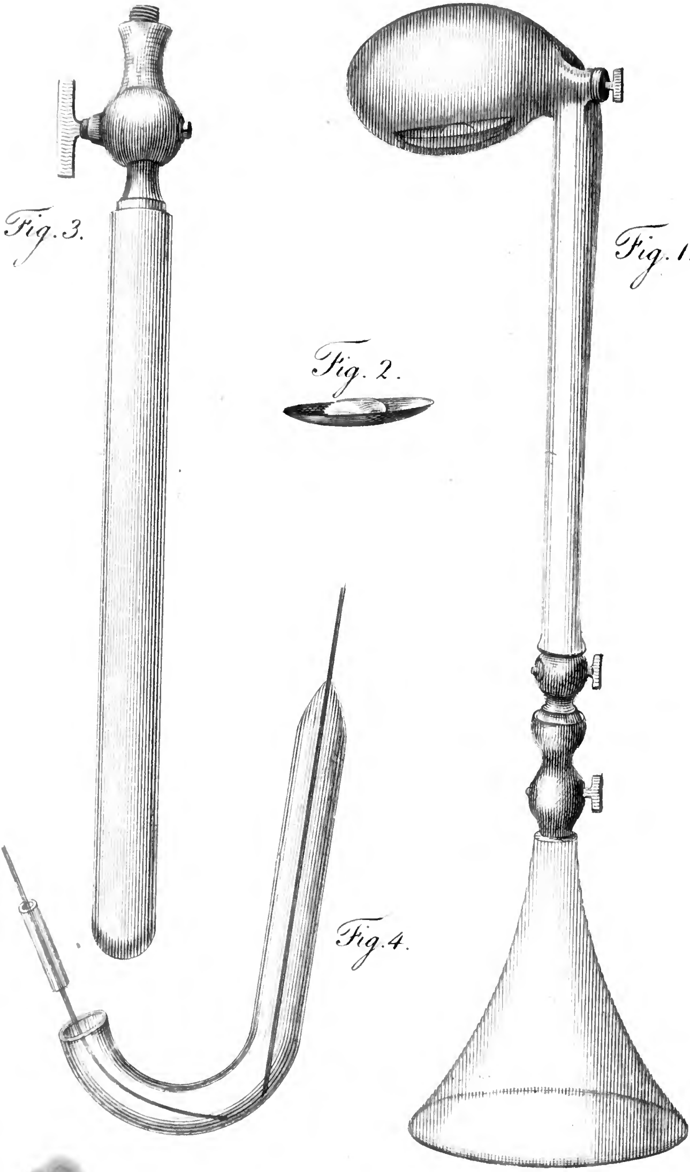


Fig. 3.

Fig. 1.

Fig. 2.

Fig. 4.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY

AND

THE ARTS.

AUGUST, 1809.

ARTICLE I.

The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous Matter, and the Acids hitherto undecompounded; with some general Observations on Chemical Theory. By HUMPHRY DAVY, *Esq. Sec. R.S. F.R.S. Ed. and M.R.I.A.* *

1. *Introduction.*

Object of the experiments.

IN the following pages, I shall do myself the honour of laying before the Royal Society an account of the results of the different experiments, made with the hopes of extending our knowledge of the principles of bodies by the new powers and methods arising from the applications of electricity to chemistry, some of which have been long in progress, and others of which have been instituted since their last session.

* Philos. Trans. for 1809, Part I. p. 39.

Their subjects.

The objects which have principally occupied my attention are the elementary matter of ammonia, the nature of phosphorus, sulphur, charcoal, and the diamond, and the constituents of the boracic, fluoric, and muriatic acids.

Among the numerous processes of decomposition, which I have attempted, many have been successful; and from those which have failed some new phenomena have usually resulted, which may possibly serve as guides in future inquiries. On this account, I hall keep back no part of the investigation, and I shall trust to the candour of the Society for an excuse for its imperfection.

The more approaches are made in chemical inquiries towards the refined analysis of bodies, the greater are the obstacles which present themselves, and the less perfect the results.

Pure substances seldom obtained.

All the difficulties, which occur in analysing a body, are direct proofs of the energy of attraction of its constituent parts. In the play of affinities with respect to secondary compounds even, it rarely occurs, that any perfectly pure or unmixed substance is obtained; and the principle applies still more strongly to primary combinations.

First methods imperfect.

The first methods of experimenting on new objects likewise are necessarily imperfect; novel instruments are demanded, the use of which is only gradually acquired, and a number of experiments of the same kind must be made, before one is obtained, from which correct data for conclusions can be drawn.

2. *Experiments on the Action of Potassium on Ammonia, and Observations on the Nature of these two Bodies.*

Oxigen in ammonia.

In thè Bakerian lecture, which I had the honour of reading before the Society, November 19, 1807, I mentioned, that in heating potassium strongly in ammonia, I found that there was a considerable increase of volume of the gas, that hidrogen and nitrogen were produced, and that the potassium appeared to be oxidated; but this experiment, as I had not been able to examine the residuum with accuracy, I did not publish. I stated it as an evidence, which I intended to pursue more fully, of the existence of oxigen in ammonia.

In

In a paper read before the Royal Society last June, which they have done me the honour of printing, I have given an account of various experiments on the amalgam from ammonia, discovered by Messrs. Berzelius and Pontin, and in a note attached to this communication, I ventured to controvert an opinion of M. M. Gay Lussac and Thenard with respect to the agency of potassium and ammonia, even on their own statement of facts, as detailed in the Moniteur for May 27, 1808.

Opinions of Gay Lussac and Thenard erroneous.

The general obscurity belonging to these refined objects of research, their importance and connection with the whole of chemical theory, have induced me since that time to apply to them no inconsiderable degree of labour and attention; and the results of my inquiries will, I trust, be found not only to confirm my former conclusions; but likewise to offer some novel views.

Apparatus.

In the first of these series of operations on the action of potassium on ammonia, I used retorts of green glass; I then, suspecting oxigen might be derived from the metallic oxides in the green glass, employed retorts of plate glass, and last of all, I fastened the potassium upon trays of platina, or iron, which were introduced into the glass retorts furnished with stop cocks. These retorts were exhausted by an excellent air pump, they were filled with hidrogen, exhausted a second time, and then filled with ammonia from an appropriate mercurial gas holder*. In this way the gas was operated upon in a high degree of purity, which was always ascertained; and all the operations performed out of the contact of mercury, water, or any substances that could interfere with the results.

Potassium used.

I at first employed potassium procured by electricity; but I soon substituted for it the metal obtained by the action of

* A representation of the instruments and apparatus is annexed.

Pl. VII. fig. 1. The retort of plate glass for heating potassium in gasses.

Fig. 2. The tray of platina for receiving the potassium.

Fig. 3. The platina tube for receiving the tray in experiments of distillation.

Fig. 4. The apparatus for taking the voltaic spark in sulphur and phosphorus.

ignited iron upon potash, in the happy method discovered by M. M. Gay Lussac and Thenard, finding that it gave the same results, and could be obtained of a uniform quality *, and infinitely larger quantities, and with much less labour and expense.

Potassium brought into contact with ammonia.

When ammonia is brought into contact with about twice its weight of potassium at common temperatures, the metal loses its lustre and becomes white, there is a slight diminution in the volume of the gas; but no other effects are produced. The white crust examined proves to be potash, and the ammonia is found to contain a small quantity of hidrogen, usually not more than equal in volume to the metal.

Heated in the gas.

On heating the potassium in the gas, by means of a spirit lamp applied to the bottom of the retort, the colour of the crust is seen to change from white to a bright azure, and this gradually passes through shades of bright blue and green into dark olive. The crust and the metal then fuse together; there is a considerable effervescence, and the crust, passing off to the sides, suffers the brilliant surface of

Cooled.

the potassium to appear. When the potassium is cooled in this state it is again covered with the white crust.

Heated a second time.

By heating a second time, it swells considerably, becomes porous, and appears crystallized, and of a beautiful azure tint; the same series of phenomena, as those before described, occur in a continuation of the process; and it is finally entirely converted into the dark olive coloured substance.

Hidrogen? evolved, and ammonia disappears.

In this operation, as has been stated by M. M. Gay Lussac and Thenard, a gas, which gives the same diminution by detonation with oxigen as hidrogen, is evolved, and ammonia disappears.

The proportion of the ammonia, which looses its elastic

The potassium probably contaminated with a little iron.

* When the potash used for procuring potassium in this operation was very pure, and the iron turnings likewise very pure and clean, and the whole apparatus free from any foreign matters, the metal produced differed very little, in its properties, from that obtained by the Voltaic battery. Its lustre, ductility, and inflammability were similar. Its point of fusion and specific gravity were, however, a little higher, it requiring nearly 130° of Fahrenheit to render it perfectly fluid, and being to water as 7960 to 10000, at 60° Fahrenheit. This I am inclined to attribute to its containing a minute proportion of iron.

form,

form, as I have found by numerous trials, varies according as the gas employed contains more or less moisture.

Less of the gas disappears when moist.

Thus eight grains of potassium, during its conversion into the olive coloured substance, in ammonia saturated with water at 63° Fahrenheit, and under a pressure equal to that of 29·8 inches of mercury, had caused the disappearance of twelve cubical inches and a half of ammonia; but the same quantity of metal acted upon under similar circumstances, except that the ammonia had been deprived of as much moisture as possible by exposure for two days to potash that had been ignited, occasioned a disappearance of sixteen cubical inches of the volatile alkali.

The inflammable gas always in proportion to the metal, and less than results from the action of water.

Whatever be the degree of moisture of the gas, the quantities of inflammable gas generated have always appeared to me to be equal for equal quantities of metal. M. M. Gay Lussac and Thenard are said to have stated, that the proportions in their experiment were the same as would have resulted from the action of water upon potassium. In my trials, they have been rather less. Thus, in an experiment conducted with every possible attention to accuracy of manipulation, eight grains of potassium generated, by their operation upon water, eight cubical inches and a half of hidrogen gas: and eight grains from the same mass, by their action upon ammonia, produced eight cubical inches and one eighth of inflammable gas. This difference is inconsiderable, yet I have always found it to exist, even in cases where the ammonia has been in great excess, and every part of the metal apparently converted into the olive coloured substance.

Properties of the substance produced by the action of ammonia on potassium.

No other account of the experiments of M. M. Gay Lussac and Thenard has, I believe, as yet been received in this country, except that in the Moniteur already referred to; and in this no mention is made of the properties of the substance produced by the action of ammonia on potassium. Having examined them minutely and found them curious, I shall generally describe them.

1. It is crystallized, and presents irregular facets, which are extremely dark, and in colour and lustre not unlike the protoxide of iron; it is opaque when examined in large masses,

masses, but is semitransparent in thin films, and appears of a bright brown colour by transmitted light.

2. It is fusible at a heat a little above that of boiling water, and if heated much higher, emits globules of gas.

3. It appears to be considerably heavier than water, for it sinks rapidly in oil of sassafras.

4. It is a nonconductor of electricity.

5. When it is melted in oxigen gas, it burns with great vividness, emitting bright sparks. Oxigen is absorbed, nitrogen is emitted, and potash, which from its great fusibility seems to contain water, is formed.

6. When brought into contact with water, it acts upon it with much energy, produces heat, and often inflammation, and evolves ammonia. When thrown upon water, it disappears with a hissing noise, and globules from it often move in a state of ignition upon the surface of the water. It rapidly effervesces and deliquesces in air, but can be preserved under naphtha, in which, however, it softens slowly, and seems partially to dissolve. When it is plunged under water filling an inverted jar, by means of a proper tube, it disappears instantly with effervescence, and the nonabsorbable elastic fluid liberated is found to be hidrogen gas.

The ponderable matter of the ammonia exists in this product

By far the greatest part of the ponderable matter of the ammonia, that disappears in the experiment of its action upon potassium, evidently exists in the dark fusible product. On weighing a tray containing six grains of potassium, before and after the process, the volatile alkali employed having been very dry, I found that it had increased more than two grains; the rapidity with which the product acts upon moisture prevented me from determining the point with great minuteness; but I doubt not, that the weight of the olive coloured substance and of the hidrogen disengaged precisely equals the weight of the potassium, and ammonia consumed.

Results of Gay Lussac and Thenard.

M. M. Gay Lussac and Thenard * are said to have pro-

* No notice is taken of the apparatus used by M. M. Gay Lussac and Thenard in the Moniteur; but from the tenour of the details, it seems that they must have operated in glass vessels in the way heretofore adopted over mercury.

cured

cured from the fusible substance, by the application of a strong heat, two fifths of the quantity of ammonia that had disappeared in their first process, and a quantity of hidrogen and nitrogen in the proportions in which they exist in ammonia, equal to one fifth more.

Different from Mr. Davy's.

My results have been very different, and the reasons will, I trust, be immediately obvious.

The fusible substance heated in hidrogen.

When the retort containing the fusible substance is exhausted, filled with hidrogen and exhausted a second time, and heat gradually applied, the substance soon fuses, effervesces, and, as the heat increases, gives off a considerable quantity of elastic fluid, and becomes at length, when the temperature approaches nearly to dull redness, a dark gray solid, which by a continuance of this degree of heat does not undergo any alteration.

Gas expelled from it by heat.

In an experiment, in which eight grains of potassium had absorbed sixteen cubical inches of well dried ammonia in a glass retort, the fusible substance gave off twelve cubical inches and half of gas, by being heated nearly to redness; and this gass analysed was found to consist of three quarters of a cubical inch of ammonia, and the remainder of elastic fluids, which when mixed with oxigen gas in the proportion of $6\frac{1}{2}$ to 6, and acted upon by the electric spark, diminished to $5\frac{1}{2}$. The temperature of the atmosphere, in this process, was 57° Fahrenheit, and the pressure equalled that of 30·1 inches of mercury.

Heated in a polished iron tube filled with hidrogen.

In a similar experiment, in which the platina tray containing the fusible substance was heated in a polished iron tube filled with hidrogen gas, and connected with a pneumatic apparatus containing very dry mercury, the quantity of elastic fluid given off, all the corrections being made, equalled thirteen cubical inches and three quarters, and of these a cubical inch was ammonia; and the residual gas, and the gas introduced into the tube being accounted for, it appeared, that the elastic fluid generated, destructible by detonation with oxigen, was to the indestructible elastic fluid, as 2·5 to 1.

In this process, the heat applied approached to the dull red heat. The mercury, in the thermometer, stood at 62° Fahrenheit, and that in the barometer at 30·3 inches.

In

Similar results in different experiments.

In various experiments on different quantities of the fusible substance, in some of which the heat was applied to the tray in the green glass retort, and in others, after it had been introduced into the iron tube; and in which the temperature was sometimes raised slowly and sometimes quickly, the comparative results were so near these, that I have detailed, as to render any statement of them superfluous.

Difference between the iron tube and green glass retort.

A little more ammonia, and rather a larger proportion of inflammable gas *, were in all instances evolved when the iron tube was used, which I am inclined to attribute to the following circumstances. When the tray was brought through the atmosphere to be introduced into the iron tube, the fusible substance absorbed a small quantity of moisture from the air, which is connected with the production of ammonia. And in the process of heating in the retort, the green glass was blackened, and I found that it contained a very small quantity of the oxides of lead and iron, which must have caused the disappearance of a small quantity of hidrogen.

Effects of moisture.

M. M. Gay Lussac and Thenard, it appears from the statement, had brought the fusible substance into contact with mercury, which must have given to it some moisture: and whenever this is the case, it furnishes by heat variable quantities of ammonia. In one instance, in which I heated the fusible substance from nine grains of potassium, in a retort that had been filled with mercury in its common state of dryness, I obtained seven cubical inches of ammonia as the first product; and in another experiment which had been made with eight grains, and in which moisture was purposely introduced, I obtained nearly nine cubical inches of ammonia, and only four of the mixed gasses.

With a proper quantity of moisture, the original quantity of ammonia would be regenerated.

I am inclined to believe, that if moisture could be introduced only in the proper proportion, the quantity of ammonia generated would be exactly equal to that which disappeared in the first process.

* The average of six experiments made in a tube of iron is 2·4 of inflammable gas to 1 of uninflammable. The average of three made in green glass retorts is 2·3 to 1.

This

This idea is confirmed by the trials which I have made, by heating the fusible substance with potash containing its water of crystallization, and muriate of lime partially dried*.

In both these cases, ammonia was generated with great rapidity, and no other gas, but a minute quantity of inflammable gas, evolved, which was condensed by detonation with oxigen with the same phenomena as pure hidrogen.

In one instance, in which thirteen cubical inches of ammonia had disappeared, I obtained nearly eleven and three quarters by the agency of the water of the potash; the quantity of inflammable gas generated was less than four tenths of a cubical inch.

In another, in which fourteen cubical inches had been absorbed, I procured by the operation of the moisture of muriate of lime nearly eleven cubical inches of volatile alkali, and half a cubical inch of inflammable gas; and the differences, there is every reason to believe, were owing to an excess of water in the salts, by which some of the gas was absorbed.

The fusible substance does not produce ammonia by heat alone.

Whenever, in experiments on the fusible substance, it has been procured from ammonia saturated with moisture, I have always found that more ammonia is generated from it by mere heat; and the general tenour of the experiments inclines me to believe, that the small quantity, produced in experiments performed in vacuo, is owing to the small quantity of moisture furnished by the hidrogen gas introduced, and that the fusible substance, heated out of the presence of moisture, is incapable of producing volatile alkali.

Gay Lussac and Thenard.

M. M. Gay Lussac and Thenard, it is stated, after having obtained three fifths of the ammonia or its elements that had disappeared in their experiment, by heating the product;

* If water, in its common form, is brought into contact with the fusible substance, it is impossible to regulate the quantity, so as to gain conclusive results, and a very slight *excess* of water causes the disappearance of a very large quantity of the ammonia generated. In potash and muriate of lime, in certain states of dryness, the water is too strongly attracted by the saline matter to be given off, except for the purpose of *generating* the ammonia.

procured

procured the remaining two fifths, by adding water to the residuum, which after this operation was found to be potash. No notice is taken of the properties of this residuum, which, as the details seem to relate to a single experiment, probably was not examined; nor as moisture was present at the beginning of their operations could any accurate knowledge of its nature have been gained.

Properties of the residuum of the fusible substance after exposure to heat.

I have made the residuum of the fusible substance after it has been exposed to a dull red heat, out of the contact of moisture, an object of particular study, and I shall detail its general properties.

It was examined under naphtha, as it is instantly destroyed by the contact of air.

1. Its colour is black, and its lustre not much inferior to that of plumbago.

2. It is opaque even in the thinnest films.

3. It is very brittle, and affords a deep gray powder.

4. It is a conductor of electricity.

5. It does not fuse at a low red heat, and when raised to this temperature, in contact with plate glass, it blackens the glass, and a grayish sublimate rises from it, which likewise blackens the glass.

6. When exposed to air at common temperatures, it usually takes fire immediately, and burns with a deep red light.

7. When it is acted upon by water, it heats, effervesces most violently, and evolves volatile alkali, leaving behind nothing but potash. When the process is conducted under water, a little inflammable gas is found to be generated. A residuum of eight grains giving in all cases about $\frac{20}{100}$ of a cubical inch.

8. It has no action upon quicksilver.

9. It combines with sulphur and phosphorus by heat, without any vividness of effect, and the compounds are highly inflammable, and emit ammonia, and the one phosphuretted and the other sulphuretted hidrogen gas, by the action of water.

A compound of nitrogen with suboxide of potassium.

As an inflammable gas alone, having the obvious properties of hidrogen, is given off during the action of potassium upon ammonia, and as nothing but gasses apparently the same

same as hidrogen and nitrogen, nearly in the proportions in which they exist in volatile alkali, are evolved during the exposure of the compound to the degree of heat which I have specified; and as the residual substance produces ammonia with a little hidrogen by the action of water, it occurred to me, that, on the principles of the antiphlogistic theory, it ought to be a compound of potassium, a little oxigen and nitrogen, or a combination of a suboxide of potassium and nitrogen; for the hidrogen disengaged in the operations of which it was the result nearly equalled the whole quantity contained in the ammonia employed; and it was easy to explain the fact of the reproduction of the ammonia by water, on the supposition, that by combination with one portion of the oxigen of the water, the oxide of potassium became potash, and by combination with another portion and its hidrogen, the nitrogen was converted into volatile alkali.

Experiments to prove this.

With a view to ascertain this point, I made several experiments on various residuums, procured in the way that I have just stated, from the action of equal quantities of potassium on dry ammonia in platina trays, each portion of metal equalling six grains.

Quantity of ammonia produced.

In the first trials, I endeavoured to ascertain the quantity of ammonia generated by the action of water upon a residuum, by heating it with muriate of lime or potash partially deprived of moisture; and after several trials, many of which failed, I succeeded in obtaining four cubical inches and a half of ammonia. In three other cases, where there was reason to suspect a small excess of water, the quantities of ammonia were three cubical inches and a half, three and eight tenths, and four and two tenths.

These experiments were performed in the iron tube used for the former process; the tray was not withdrawn; but the salt introduced in powder, and the apparatus exhausted as before, then filled with hidrogen, and then gently heated in a small portable forge.

The compound introduced into oxigen gas.

Having ascertained what quantity of ammonia was given off from the residuum, I endeavoured to discover what quantity of nitrogen it produced in combustion, and what quantity of oxigen it absorbed. The methods that I employed,

ployed, were by introducing the trays into vessels filled with oxigen gas over mercury. The product often inflamed spontaneously, and could always be made to burn by a slight degree of heat.

Oxigen absorbed and nitrogen evolved.

In the trial that I regard as the most accurate, two cubical inches and a half of oxigen were absorbed, and only a cubical inch and one tenth of nitrogen evolved.

Surprised at the smallness of the quantity of the nitrogen, I sought for ammonia in the products of these operations; but various trials convinced me, that none was formed. I examined the solid substances produced, expecting nitrous acid; but the matter proved to be dry potash, apparently pure, and not affording the slightest traces of acid.

The quantity of nitrogen existing in the ammonia, which this residuum would have produced by the action of *water*, supposing the volatile alkali decomposed by electricity, would have equalled at least two cubical inches and a quarter.

Exposed to nascent oxigen, still the quantity of nitrogen small.

I heated the same proportions of residuum with the red oxide of mercury, and the red oxide of lead in vacuo, expecting that when oxigen was supplied in a gradual way, the result might be different from that of combustion; but in neither of these cases did the quantity of nitrogen exceed a cubical inch and a half.

But on what could this loss of nitrogen depend; had it entered into any unknown form with oxigen, or did it not really exist in the residuum in the same quantity, as in the *ammonia* produced from it?

Residuum exposed to intense heat.

I hoped that an experiment of exposing the residuum to intense heat might enlighten the inquiry. I distilled one of the portions which had been covered with naphtha, in a tube of wrought platina made for the purpose. The tube had been exhausted and filled with hidrogen, and exhausted again, and was then connected with a pneumatic mercurial apparatus. Heat was at first slowly applied, till the naphtha had been driven over. It was then raised rapidly by an excellent forge. When the tube became cherry red, gas was developed; it continued to be generated for some minutes. When the tube had received the most intense heat, that

that could be applied, the operation was stopped. The quantity of gas collected, making the proper corrections and reductions, would have been three cubical inches and a half at the mean temperature and pressure. Twelve measures of it were mixed with six of oxigen gas, the electrical spark was passed through the mixture; a strong inflammation took place, the diminution was to three measures and a half, and the residuum contained oxigen. This experiment was repeated upon different quantities with the same comparative results.

The gas detonated.

In examining the platina tube, which had a screw adapted to it at the lower extremity, by means of which it could be opened, the lower part was found to contain potash, which had all the properties of the pure alkali, and in the upper part there was a quantity of potassium. Water poured into the tube produced a violent heat and inflammation; but no smell of ammonia.

In the tube, potash and potassium.

This result was so unexpected and so extraordinary, that I at first supposed there was some source of errour. I had calculated upon procuring nitrogen as the only aeriform product; I obtained an elastic fluid, which gave much more diminution by detonation with oxigen, than that produced from ammonia by electricity.

I now made the experiment, by heating the entire fusible substance from six grains of potassium, which had absorbed twelve cubical inches of ammonia, in the iron tube, in the manner before described. The heat was gradually raised to whiteness, and the gas collected in two portions. The whole quantity generated, making the usual corrections for temperature and pressure, and the portion of hidrogen originally in the tube, and the residuum, would have been fourteen cubical inches and a half at the mean degree of the barometer and thermometer. Of these, nearly a cubical inch was ammonia, and the remainder a gas, of which the portion destructible by detonation with oxigen was to the indestructible portion, as 2·7 to 1.

Gas from the whole of the fusible substance heated.

The lower part of the tube, where the heat had been intense, was found surrounded with potash in a vitreous form; the upper part contained a considerable quantity of potassium.

Solid results.

In

More than one third of the potassium revived.

In another similar experiment, made expressly for the purposes of ascertaing the quantity of potassium recovered, the same elastic products were evolved. The tube was suffered to cool, the stop-cock being open in contact with mercury, it was filled with mercury, and the mercury displaced by water; when two cubical inches and three quarters of hidrogen gas were generated, which proved, that at least two grains and a half of potassium had been revived.

Calculation of the results.

Now, if a calculation be made upon the products in these operations, considering them as nitrogen and hidrogen, and taking the common standard of temperature and pressure, it will be found, that, by the decomposition of 11 cubical inches of ammonia equal to 2·05 grains, there are generated 3·6 cubical inches of nitrogen equal to 1·06 grains, and 9·9 cubical inches of hidrogen, which, added to that disengaged in the first operation equal to about 6·1 cubical inches, are together equal to ·382 of a grain; and the oxigen added to 3·5 grains of potassium would be ·6 of a grain, and the whole amount is 2·04 grains; and 2·05 — 2·04 = ·01. But the same quantity of ammonia, decomposed by electricity, would have given 5·5 cubical inches of nitrogen equal to 1·6 grain, and only 14 cubical inches of hidrogen * equal to ·33, and allowing the separation of oxigen in this process in water, it cannot be estimated at more than ·11 or ·12.

Nitrogen lost and oxigen and hidrogen produced.

So that, if the analysis of ammonia by electricity at all approaches towards accuracy; in the process just described, there is a considerable loss of nitrogen, and a production of oxigen and inflammable gas.

Nitrogen generated when water employed.

And in the action of water upon the residuum, in the experiment page 252, there is an apparent generation of nitrogen.

How can these extraordinary results be explained?

Suppositions to explain this.

The decomposition and composition of nitrogen seem proved, allowing the correctness of the data; and one of its elements appears to be oxigen; but what is its other elementary matter?

* See Phil. Trans. 1808, p. 40, or Journal, vol. xx, p. 328.

Is

Is the gas, that appears to possess the properties of hidrogen, a new species of inflammable aeriform substance?

Or has nitrogen a metallic basis, which alloys with the iron or platina?

Or is water alike the *ponderable* matter of nitrogen, hidrogen, and oxigen?

Or is nitrogen a compound of hidrogen with a larger proportion of oxigen than exists in water?

These important questions, the two first of which seem the least likely to be answered in the affirmative, from the correspondence between the weight of the ammonia decomposed and the products, supposing them to be known substances, I shall use every effort to solve by new labours, and I hope soon to be able to communicate the results of farther experiments on the subject to the Society.

Ammonia decomposed in the experiment, and potassium not a compound of hidrogen and potash.

As the inquiry now stands, it is however sufficiently demonstrative, that the opinion, which I had ventured to form respecting the decomposition of ammonia in this experiment, is correct; and that M. M. Gay Lussac's and Thenard's idea of the decomposition of the potassium, and their theory of its being compounded of hidrogen and potash, are unfounded.

For a considerable part of the potassium is recovered unaltered, and in the entire decomposition of the fusible substance, there is only a small excess of hidrogen above that existing in the ammonia acted upon.

The mere phenomena of the process likewise, if minutely examined, prove the same thing.

After the first slight effervescence, owing to the water absorbed by the potash formed upon the potassium during its exposure to the air, the operation proceeds with the greatest tranquillity. No elastic fluid is given off from the potassium; it often appears covered with the olive coloured substance, and, if it were evolving hidrogen, this must pass through the fluid; but even to the end of the operation, no such appearance occurs.

The crystallized and spongy substance, formed in the first part of the process, I am inclined to consider as a combination of ammonia and potassium, for it emits a smell of ammonia

ammonia when exposed to air, and is considerably lighter than potassium.

Potassium does not absorb hidrogen, but is soluble in it.

I at first thought, that a solid compound of hidrogen and potassium might be generated in the first part of this operation: but experiments on the immediate action of potassium and hidrogen did not favour this opinion. Potassium, as I ventured to conclude in the Bakerian Lecture for 1807*, is

Hidrogen said to be absorbed by potassium.

* M. M. Gay Lussac and Thenard seem to be of a different opinion. In the Moniteur, to which I have so often referred, it is related, that these distinguished chemists, by exposing hidrogen to potassium at a high temperature, found that the hidrogen was absorbed, and that it formed a compound with the potassium of a light gray colour, from which hidrogen was capable of being obtained by the action of water or mercury.

Not in Mr. Davy's experiments.

After a number of trials, I have not been able to witness this result. In an experiment which I made in the presence of Mr. Pepys, and which I have often repeated, and twice before a numerous assembly, in retorts of plate glass, four grains of potassium were heated in fourteen cubical inches of pure hidrogen. At first, white fumes arose and precipitated themselves in the neck of the retort. When a considerable film of the precipitate had collected, its colour appeared a bright gray, and after the first two or three minutes, it ceased to be formed.

The bottom of the retort was heated to redness, when the potassium began to sublime and condense on the sides.

The process was stopped, and the retort suffered to cool. The absorption was not equal to a quarter of a cubical inch. When the retort was *broken*, the gas, in passing into the atmosphere, produced an explosion with most vivid light, and white fumes. The potassium remaining in the retort, and that which had sublimed, seemed unaltered in their properties.

The grayish substance inflamed by the action of water, but did not seem to be combined with mercury. I am inclined to attribute its formation to the agency of moisture suspended in the hidrogen, and to consider it as a triple compound of potassium, oxigen, and hidrogen.

Potassium heated in hidrogen.

When potassium is heated in a gas containing hidrogen, and from $\frac{1}{15}$ to $\frac{1}{30}$ of common air, it is formed in greater quantities, and a crust of it covers the metal, and in the process there is an absorption both of hidrogen and oxigen. It is likewise produced in experiments on the generation of potassium by exposing potash to ignited iron, at the time (I believe) that common air is admitted, during the cooling of the tube.

It is nonconducting, inflames spontaneously in air, and produces potash and aqueous vapour by its combustion.

When

is very soluble in hidrogen; but, under common circumstances, hidrogen does not seem to be absorbable by potassium.

When potassium is heated in hidrogen in a flint glass retort, or even for a great length of time in a green glass retort, there is an absorption of the gas; but this is independent of the presence of potassium, and is owing to the action of the metallic oxides in the glass upon the hidrogen. Hidrogen absorbed by the oxides in the glass.

If a solid compound of hidrogen and potassium could be formed, we might expect its existence in the experiment with the gun barrel, in which potassium is exposed to hidrogen at almost every temperature; but the metal formed in this process, when proper precautions are taken to exclude carbonaceous matters, is uniform in its properties, and generates, for equal quantities, equal proportions of hidrogen by the action of water.

The general phenomena of this operation show indeed, that the solution of potassium in hidrogen is intimately connected with the general principle of the decomposition; and confirm my first idea of the action of the two bodies.

Hidrogen dissolves a large quantity of potassium by heat, but the greater portion is precipitated on cooling. The attractions which determine the chemical change seem to be that of iron for oxigen, of iron for potassium, and of hidrogen for potassium; and in experiments, in which a very intense heat is used for the production of potassium by iron, I have often found, that the gas which comes over, though it has passed through a tube cooled by ice, inflames spontaneously in the atmosphere, and burns with a most brilliant light, which is purple at the edges, and throws off a dense vapour containing potash.

Sodium appears to be almost insoluble in hidrogen, and this seems to be one reason why it cannot be obtained, except in very minute quantities, in the experiment with the gun barrel. Sodium nearly insoluble in hidrogen.

Sodium, though scarcely capable of being dissolved in hidrogen alone, seems to be soluble in the compound of hidrogen and potassium. By exposing mixtures of potash and soda to ignited iron I have obtained some very curious alloys; which, whether the potassium or the sodium was in excess, were fluid at common temperatures. The compound containing an excess of potassium was even light than potassium (probably from its fluidity). All these alloys were in the highest degree inflammable. When a globule of the fluid alloy was touched by a globule of mercury, they combined with a heat that singed the paper upon which the experiment was made, and formed, when cool, a solid so hard, as not to be cut by a knife. Curious alloys.

(To be continued in our next.)

II.

On the Production of an Acid and an Alkali from pure Water by Galvanism. In a Letter from Mr. CHARLES SYLVESTER.

To Mr. NICHOLSON.

SIR,

Soda and muriatic acid produced from water by galvanism.

IT is now a long time since I had the pleasure of communicating any thing to your valuable periodical work, although I was under a promise to send you something decisive on the subject of the production of soda and muriatic acid, from pure water, by galvanism. I should not at present have ventured to have offered any thing on this subject, knowing, that the tide of opinion must have gone with the decisions of Mr. Davy, who has said, that the acid and alkali are produced from foreign matter in the water, or in the vessels employed; had not the truth and consequent reasonings of my experiments been strongly supported by many recent facts, brought forward by Mr. D. himself.

Mr. Davy's experiments made on oxides of hidrogen.

All the experiments, in which Mr. Davy has produced the apparent base of an alkali, an earth, or even acid, are nothing more than degrees of the same process, by which the alkali is produced when pure water is exposed to the galvanic influence; and it is equally evident, that all the bodies he has, in these experiments, operated upon, are oxides of hidrogen. I have not the least hesitation in saying, that the acid and alkali can be produced, from pure water, in such abundance as not to admit a doubt of their being derived from the water, or the apparatus. The importance of the electrical agency in chemical processes appears principally to consist in hidrogen and oxigen being furnished in their nascent and pure form; for it will be recollected, that in all experiments, in which the alkalis and the earths have appeared to be decomposed, the presence of water has always been essential to the changes produced.

Acid and alkali produced in abundance.

Electrical agency in chemical processes.

Water with oxigen forms acids; with

It is therefore probable, that water with different portions of oxigen forms acid products; and with hidrogen,

hidrogen, alkalis, earths, and metals.

alkalis, earths, and metals. In the experiment, where pure water is exposed to the galvanic influence, separated into two portions by some moist conductor, the oxigen is presented in its nascent form, and an acid is produced, from that substance combining with the water; and at the point where the hidrogen is presented, an alkali is formed, by a similar fixation of hidrogen. In the pretended decomposition of potash, the alkali combines with an extra dose of hidrogen, forming the metallic globulus. And when a metal was said to be produced from ammonia, forming an alloy with potassium remarkable for its little specific gravity, the effect could only be attributed to that metal combining with a still greater portion of hidrogen.

Mr Davy's doctrine true and consistent. Only two kinds of simple matter.

The electrical doctrine of Mr. Davy is so replete with truth and consistency, that I am every day more pleased with it. It would seem, that we have only two kinds of simple matter; one something like oxigen, possessing the effects of negative electricity in the greatest degree; the other a general inflammable substance of the nature of hidrogen, endowed with positive electricity: that each of these bodies has a constant repulsion between their homogeneous particles, and hence is permanently elastic; that equal portions of these bodies combined would constitute a body of the greatest possible density, from the attraction being at a maximum: and that, as one of them predominates, the attraction becomes less. Hence it appears, that the particles of simple matter are repellent of each other, and that no solid body can be considered a simple body.

No solid a simple body.

A friend of mine intends soon to favour you with a more extensive essay on this subject.

If you think the above observations will at all interest the readers of your work, their insertion in your next will much oblige,

Sir,

Your humble servant,

Derby, *June* 23, 1809. CHARLES SYLVESTER.

This letter came too late for insertion last month. It seems proper to notice, that Mr. Davy states the decompo-

sition of potash &c., where no water was present. With regard to theories, there must always be great difficulty when inductions are made and generalized beyond the support afforded by the facts. Specific facts duly arranged in support of each other are the great desiderata of science. We possess many, the happy acquisition of our own time, but we are in want of many more.

W. N.

III.

Account of the Decomposition and Recomposition of Boracic Acid. By Messrs. GAY LUSSAC *and* THENARD *.

Decomposition of boracic acid announced.

ON the 21st of June last we announced in a note read at the Institute, and we published in the *Bulletin de la Société Philomatique* for July, that by treating the fluoric and boracic acids with the metal of potash we obtained results, which could only be explained by admitting these acids to be compounds of a combustible substance and oxigen. However, as we had not recomposed them, we added, that we did not give this composition as completely demonstrated. Since that time we have continued and varied our researches, and are now able to assert, that the composition of the boracic acid is no longer problematical. In fact, we can decompose this acid and recompose it at pleasure.

Method in which it was decomposed.

To decompose it, we put equal parts of the metal and very pure and well vitrified boracic acid into a copper tube, to which a curved glass tube is fitted. The tube of copper is placed in a small furnace, and the extremity of the glass tube in a jar filled with mercury. The apparatus being thus arranged, the copper tube is heated gradually, till it is slightly red hot. In this state it is kept for some minutes. The operation being then finished, it is cooled, and the

* Journal de Physique for November, 1808, Vol LXVII, p. 393. Mr. Davy's experiments on the boracic acid will appear in the course of the paper, of which the commencement is given in our present number. See also Journal, Vol. XX, p. 391, and Vol. XXI, p. 375.

matters

matter taken out. The following are the phenomena observed in this experiment.

Phenomena observed in the experiment.

When the temperature is about 150° [302° F.], the mixture on a sudden grows highly red, as may be seen in a striking manner by using a glass tube. There is even so much heat produced, that the glass tube partly melts, and sometimes breaks, and the air of the vessels is almost always expelled with force. From the beginning of the experiment to the end, nothing is disengaged but atmospheric air, and a few bubbles of hidrogen gas, not answering to a fiftieth part of what the metal employed would give out by means of water. All the metal constantly disappears in decomposing part of the boracic acid; and the two substances are converted by their reciprocal action into an olive gray matter, which is a mixture of potash and the radical of the boracic acid. This mixture is extracted from the tube by pouring in water, and heating it gently; and the boracic radical is separated by repeated washing with warm or cold water. Before this washing it is advisable to saturate the alkali contained in the mixture with muriatic acid: for it appears, that the boracic radical can become oxided, and then dissolve in the alkali, to which it gives a very deep colour. What does not dissolve is the radical itself, which possesses the following properties.

Properties of the base of boracic acid.

It is of a greenish brown colour, fixed, and insoluble in water. It has no taste; and no action on infusion of litmus or sirup of violets. Mixed with oximurate of potash, or nitrate of potash, and projected into a red hot crucible, a vivid combustion ensues, one of the products of which is the boracic acid. When it is treated with nitric acid, a great effervescence takes place, even in the cold: and when the fluid is evaporated, a great deal of the boracic acid is obtained. But of all the phenomena produced by the boracic radical in its contact with different substances, the most curious and most important are those it exhibits with oxigen.

This base, heated in oxigen gas,

On projecting 3 decig. [$4\frac{1}{2}$ grs.] of boracic radical into a silver crucible scarcely at a dull red heat; and covering the crucible with a jar holding about a litre [a wine quart], filled wish oxigen gas, and placed over mercury; a combustion

tion of the most instantaneous kind takes place, and the mercury rises with such rapidity half way up the jar, as to raise it forcibly. In this experiment however, the combustion of the boracic radical is far from complete. What prevents this is, that the radical is at once converted entirely into the state of a black oxide, the existence of which we think we have perceived; and the external parts of this oxide passing afterwards to the state of boracic acid, they melt, and thus defend the interior parts from the contact of the oxigen. Accordingly to burn them completely it is necessary, to wash them, and place them afresh in contact with oxigen gas, still at a cherry red heat, but then they burn with less violence, and absorb less oxigen, than the first time, because they are already oxided: and still the external parts, passing to the state of boracic acid, which melts, prevent the combustion of the interior parts; so that to convert them all into boracic acid, they must be subjected to a great number of successive combinations, and as many washings.

first oxided,

then converted into boracic acid.

In all these combustions a fixation of oxigen constantly takes place, without any gas being disengaged; and they all afford products so acid, that, in treating these products with boiling water, boracic acid is obtained after suitable evaporation and refrigeration, a specimen of which we present to the Institute.

Oxigen fixed, but no gas evolved.

Lastly, the boracic radical comports itself in air precisely as in oxigen, with this difference only, that the combustion is less vivid.

Burns less vividly in common air.

From these experiments it follows, that the boracic acid is composed of oxigen and a combustible substance. Every thing convinces us, that this substance, for which we propose the name of *bore*, is of a peculiar nature, and ought to be ranked with phosphorus, carbon, and sulphur: and we presume, that, to acquire the state of boracic acid, it demands a large quantity of oxigen; but, before attaining this state, it becomes a black oxide.

The base a combustible substance, not metallic.

Note, by the Authors.

Former trials

Several chemists have made experiments on the decomposition

sition of boracic acid, whence they have deduced different consequences. to decompose boracic acid.

Fabroni asserted, that this acid was only a modification of the muriatic. *See* Fourcroy's Chemistry, art. Boracic Acid.

In the Annales de Chimie, vol. XXXV, p. 202, we find a long series of experiments on the phenomena exhibited by boracic acid on treating it with oximuriatic acid. These experiments are by Crell, who inferred from them, that carbon was one of the elements of this acid. Supposed to contain carbon.

Lastly, Mr. Davy, subjecting moistened boracic acid to the Voltaic pile, observed traces* of a black combustible matter at the negative pole; but he says, that being occupied in researches upon the alkalis, he was unable to follow up this observation. *See* Mr. Davy's paper, which arrived in France two months ago, and an abstract of which was inserted in the *Bulletin de la Société Philomatique* for the month of November.

Thus hitherto the principles of the boracic acid were not known. It is true, we had announced to the Institute, that this acid contained oxigen, and consequently a combustible substance; but, as we had not recomposed it, we did not consider its nature as determined.

IV.

On the Influence of Galvanic Electricity on the Transition of Minerals; read at the Meeting of the Mathematical and Physical Class of the Institute, the 13th of July, 1807; by Mr. GUYTON†.

ON examining five years ago a native oxide of antimony found in the province of Gallicia, which had been sent me by Mr. Angulo, inspector general of the mines of Andalusia, Native oxide of antimony from a sulphuret.

* Mr. Davy's own expressions are: "I find, that a dark coloured combustible matter is evolved at the negative surface." *See* Journal, vol. XX, p. 321.

† Annales de Chimie, vol. LXIII, p. 113.

sia, I was led to consider this mineral as a transition from the state of a sulphuret to that of almost a pure oxide, which could have been effected only by the decomposition of water, determined by a subterranean electricity precisely similar to that we obtain in Volta's apparatus.

This shown by its appearance.

The external appearance of this mineral, which still evidently exhibits the structure of native crystallized sulphuret of antimony, and even in some parts the remains of a metallic lustre, leaves no doubt, that its entire mass was originally a sulphuret of antimony, the particles of which had undergone the slow and successive action of some agent, that had altered their composition, without disturbing their respective arrangement, precisely as we see in petrified wood, that retains its organization.

The principle applicable to other fossils.

The proofs on which I grounded this explanation, and the applications I have made of this principle to the formation of other fossils, as the pyrites termed hepatic, gray copper ore, &c., have been detailed in a paper inserted in the Journal of the Polytechnic School, for July, 1802, p. 308.

Mr. Davy's ideas similar.

Mr. Davy's views on the same subject, given at the end of his excellent Bakerian lecture read to the Royal Society on the 20th of November, 1806, where he speaks of the slow and silent operations of natural electricity even on the mineral system*, inspired me with the idea of endeavouring to corroborate the inferences drawn from my former results, by performing the experiments with the more powerful apparatus, which we at present possess.

Experiment to confirm the fact.

Messrs. Hachette and Clement were so good as to assist me in this undertaking. We formed a battery of 64 plates of copper and zinc, 15 cent. [near 6 inches] long by 10 cent. [near 4 inches] broad, affording a surface of 9600 cent. or about 1260 French inches square.

Apparatus.

This battery was arranged in Mr. van Marum's manner, that is, distributed in four piles, the first two of which were placed in a plate of copper with its edges turned up, and supported by an insulator. The pasteboards placed between pairs of metal were wetted with a strong solution of soda.

Sulphuret of

A piece of sulphuret of antimony was placed in a small glass

* See Journal, vol. XIX, p. 62.

glass two thirds full of distilled water, and a communication was established between the water and the two poles of the battery by means of two slips of platina.

antimony exposed to it.

As soon as the bubbles began to announce the decomposition of the water, a slight smell of sulphuretted hidrogen was perceptible. In two hours this smell was very strong, the water had assumed a yellow tint, and the surface of the sulphuret of antimony appeared of a deeper yellow, and as it were iridescent.

Sulphuretted hidrogen evolved.

The slips of platina from the two poles were first fixed at some distance from the sulphuret; afterward they were brought near enough to touch it, and the acceleration of the disengagement of bubbles showed, that the activity of the battery had not slackened.

Silver tarnished by it.

After the expiration of four hours, the smell of sulphuretted hidrogen was perceptible at a distance. A slip of silver, well cleaned, being placed on the edge of the glass without touching the water, was in a few minutes covered with a deep black coating. A drop of the water in the glass immediately formed a white precipitate in a solution of acetate of lead.

Acetate of lead precipitated.

That part of the slip of platina, which was connected with the negative pole and immersed in the water, was black: and that which communicated with the positive pole had a slight yellow incrustation.

The platina tarnished.

The battery having lost almost the whole of its activity at the expiration of eight hours, we attempted to take the piece of sulphuret out of the water; but the motion separating part of the yellow powder that covered it, to collect this we were obliged to throw the whole upon a filter.

The sulphuret covered with yellow powder,

This powder, dried in the air, exhibited the same reddish yellow tint as the native oxide of the province of Gallicia; and the fragment still retained evident traces of it on several points of its surface, when scarcely any remains of metallic lustre were distinguishable.

resembling the native oxide,

Hence we may presume to give this product of our imitation of the processes of nature as differing from the models she presents us only because the portion decomposed had not reached the same depth, and acquired the same consistency; in other words, because the result of an operation

and differing only from the difference of the operations.

tion of a few hours cannot be perfectly similar to that of another, the duration of which depends on a uniform succession of agents, and the slowness of which prevents all possibility of its being disturbed.

Nothing but galvanic electricity could have wrought the change.

If now we consider, that no one of the substances, which we may reasonably presume to exist in the bowels of the Earth, acts in a similar manner, or produces the same changes in sulphuret of antimony when once formed, as I have shown in the paper printed in the Journal of the Polytechnic school, there appears to me no doubt, that the transition of the sulphuret of antimony of the province of Gallicia to the state of native oxide (in which it loses more than 0·17 of sulphur, and acquires 0·18 of oxigen) results directly from the decomposition of water by galvanic electricity. If it be strictly possible for the same effect to be produced by a different cause, it is certain, that the instances are much more rare, than is commonly supposed; and that the greater number appears to belong to this class only because we confound the remote with the immediate cause, the process with the chemical action, the form of the agent with the nature, and, if I may be allowed the expression, the handle with the tool. But when the effect is characterised, as in this particular case, by circumstances that imprint on it the seal of a distinct cause, and excludes any other known cause, we have not a probability only, the certainty of the cause is equal to the certainty of the effect.

Farther proved,

We have since sought for new proofs of this conclusion, extending our experiments to other minerals, where the signs of a transition of this kind were most manifest.

on sulphuret of iron,

Sulphuret of iron, poor in metal, hard, compact, and of great lustre, merited attention in this point of view more particularly, because the pyrites of Berezoff, which is found in the same state of alteration, in its primitive state resists the action of the most powerful solvents, yielding only to the nitric acid and the nitro-muriatic.

and gray silver ore.

On a pyrites of this kind, and the gray silver ore (crystallized *fahlerz)*, we endeavoured to produce analogous alterations.

These exposed in water to a battery, were

Being exposed in distilled water to the action of the same battery, and communications established in a similar manner,

manner, the smell of sulphuretted hidrogen was perceived, and the water rendered turbid; the slips of platina were coloured, as in the former experiment, black on the negative sides, and brownish yellow on the positive; the water, which was strongly acid, precipitated acetate of lead; and the fragments of the sulphurets were left in a state of division, almost pulverulent, and covered with pellicles of a dull colour and without lustre.

acted upon in a similar manner.

The sulphuret of iron in particular exhibited a very striking alteration on its surface. Having attached the conductors before water was put into the glass, the sulphuret was vividly inflamed; which astonished us the more because in a preceding experiment a fragment of transparent native sulphur did not exhibit the least sign of inflammation, when touched with the platina exciter under a jar filled with oxigen gas, though the battery was powerful enough to burn the iron wire.

The sulphuret of iron inflamed.

It even appeared to us, in the last experiment, that the inflammation of the sulphuret took place instantaneously after it had been covered with water; but the effect was so rapid, that we dare not assert this as a certainty.

We purpose to pursue these experiments, and in the mean time I think I may conclude, that those of which I have just given an account, while they confirm my explanation of the transition of brilliant crystallized sulphuret of antimony to the state of an earthy yellow oxide, without losing its configuration, afford a new mean for interrogating nature respecting the composition of bodies, the proportions of their component parts, and the succession of changes effected in their combinations. The desulphuration of ores is one of the most important points in metallurgy; and if, in the present state of our knowledge, we can scarcely discern any possibility of availing ourselves of this mean in processes in the large way, those of assaying cannot fail to derive more certain results from its application.

These experiments to be pursued.

V.

V.

On Artificial Sandstones, that have undergone a regular Contraction in the Fire; by Mr. ALLUAU*.

Artificial sandstone separated into prisms.

ON examining with Mr. Leopold Chevaillers the scoriæ produced in the operation of parting bell metal, which was performed under his direction at Limoges, I found masses of artificial sandstone, which by a regular contraction had divided itself into prisms, precisely resembling the basaltic columns, that exist in all volcanic countries.

Composition of the stone.

These sandstones, which served as a cupel to the melting furnace, are composed of a fine grained sand, the remains of granites; the other component parts of which had been decomposed. To separate them, and obtain the purest siliceous grains, they were carefully washed and decanted, they were afterward mixed with water loaded with clay, to impart to them the body requisite for their use; and a little charcoal powder was added, which, diminishing the points of contact between the siliceous particles and the metallic oxides, rendered them less vitrescible, and thus prolonged the duration of the cupel of the furnace.

Manner in which it was formed.

To form it, a stratum of this mixture 15 to 20 cent. [6 or 8 inches] thick was placed on the floor of the furnace, and strongly beaten down as it was gradually dried by a gentle heat. After being used a certain time, it was necessary to renew the whole stratum, and all the sandstones arising from it had experienced the same contraction.

Its texture.

The upper part of these prismatic sandstones is covered with a scorified metallic stratum 4 or 5 cent. [about $1\frac{1}{2}$ or 2 inches] thick, that serves to hold together all the prisms, which are notwithstanding easily separable. The degree of heat has been more intense near this stratum, than in the inferior part: accordingly the sandstone there is harder and more compact, being difficult to penetrate; while the other extremity of the prism is easily crumbled by the fingers. The fire however has been sufficiently violent through-

* Abridged from the Journal de Physique, vol. LXV, p. 228.

out

out the whole thickness of the mass, to vitrify the metallic fragments disseminated through the sand.

These prisms extend to the length of 15 cent. [5·9 inch.] and from 1 to 2 or even 3 [0·39, to 0·78, or 1·18 of an inch] in diameter. They are parallel to each other, and have their axis constantly perpendicular to the metallic stratum that covers them. Though the number of their sides is not constant, they are most frequently six. Their edges are sharp, and pretty straight. Their faces are not strictly planes, but a little concave; and, what is remarkable, they appear to have been more powerfully heated than the interior of the prism, a circumstance I conceive to be ascribed to the last molecules of caloric, which have escaped as through so many channels by the clefts or intervals, that were formed between the adjacent faces of the prisms. Figure.

When these sandstones have not been so strongly heated, the aggregation and prismatic division is not so well characterised. Then too the charcoal, deprived of the air necessary for its combustion, has arranged itself in longitudinal zones parallel with the axis of the prism, and so as to leave between them intervals of 2 or 3 millim. [0·787 or 1·18 of a line]. This singular phenomenon appears to me occasioned by the caloric, which, absorbed by the metallic stratum, taking the shortest course to reach it, and finding itself stopped in its progress by the particles of charcoal equally disseminated through the mass of sand, pushed them aside to the right and left by imperceptible degrees to open itself a passage, and has thus dispersed them in little parallel strata or threads, as if they had yielded to the laws of affinity, which always tend to bring together homogeneal particles, when suspended by a fluid in a suitable state of rest. In some cases the charcoal arranged in parallel strata.

If we invert one of these masses of sandstone, it is a very good representation of the bottom of a basaltic stratum. In short it is impossible to have a more perfect model of its mechanical division*. The sandstone resembles basaltes,

Naturalists have already remarked clays, that have undergone a regular contraction in the fire: but, beside that silex more than any thing before observed.

* The piece I preserve in my collection is about 4 dec. [15 ½ inches long,] and 2 dec. [7 inches and three quarters] broad.

forms

forms more than nine tenths of the mass of this sandstone of Mr. Chevaillers, this effect had not been observed in such a constant manner on such large blocks, as Dolomieu said in speaking of the configuration of basaltes. An effect so frequently repeated must have its causes.

Reflections on the configuration of basaltes.

Basaltes first supposed a crystallization.

At a time when men were ignorant of the first principles of crystallography, and but few crystals were known, it was difficult certainly, to avoid confounding with them solids that exhibited some external appearance of their regularity. Thus Cronstedt, Wallerius, and other celebrated naturalists thought basaltic prisms were the direct products of crystallization.

Romé de Lisle corrected this mistake.

Romé de Lisle at first adopted the mistake of his predecessors: but he had scarcely raised the veil, that enveloped the mechanism of crystallization, when he sought for another cause of the prismatic division of basaltes, and then the happy idea of a contraction offered itself to his mind.

Haüy.

But since the genius of Haüy has developed the theory of crystallization in such a learned manner, may we not be astonished still to find naturalists, who are desirous of assimilating basaltic columns with the productions of regular aggregation?

Objection to basaltic prisms being formed by cooling,

Setting aside therefore every idea of the crystallization of basaltes, I shall confine myself to the refutation of a slight objection of its partizans to the numerous proofs of its contraction, and I shall attempt to follow its mechanism, in examining the laws to which bodies are subjected in cooling.

from their regularity.

They admit, that the cooling of the basaltes must have occasioned divisions, that would naturally give rise to some forms; but they add, that these forms, resulting from chance, and a thousand different accidents, could only be very irregular, and not produce vast columns, as remarkable for their regularity as for the uniform arrangement that characterises their extensive masses.

But they ought to be regular.

But why should these forms depend on accidents guided by the hand of chance? I can conceive no reason for this; since, if the cause of the contraction be constant, and if the manner in which it operates be always the same, ought not

not their results to bear the stamp of this uniformity of circumstances? Do not the cracks of clay dried by a scorching sun sometimes exhibit polygons nearly regular? Do not the cracks in the glaze of pottery, which superficially examined appear destitute of symmetry, resemble on closer inspection a kind of mosaic issuing from the hand of a single artist?

Regular cracks in enamel not from crystallization.

Mr. Patrin even mentions a piece of ancient enamel in the collection of Mr. Dolomieu, the surface of which exhibits throughout hexagonal figures, representing in miniature a horizontal section of a basaltic causeway. But who can conceive with him, that these hexagons are the effect of crystallization? Is it not evident, that the metallic base, on which the enamel rests, being capable of greater dilatation, may under various circumstances have occasioned cracks, the unusual regularity of which gives at first sight an erroneous idea of their causes?

The cooling of basaltes accelerated by something that quickly absorbs heat.

The basaltic prisms then are the result of a regular contraction, and the hypothesis of Dolomieu, which ascribes it to a refrigeration accelerated by the contact of a body that quickly imbibes caloric, agrees perfectly with the division of the sandstone of Mr. Chevaillers, the surface of which is covered with a scorified metallic stratum serving as a conductor to the caloric.

Caloric an agent in its formation.

If geologists be not agreed on the formation of basaltes, they cannot refuse to admit, that caloric performs one of the principal parts in it; whether it act alone, or in concert with other solvent gasses, known or unknown to naturalists: and the latter, as they are extricated, may furnish analogous results.

Effects of a sudden cessation of its agency.

When a body is strongly heated, if the action of caloric come to cease suddenly, the body experiences the most intense degree of heat at the instant when the caloric escapes. In fact, the caloric, rushing rapidly toward the body that absorbs it in proportion to the strength of its affinity, accumulates on the parts which it traverses as a powder does a sieve, and sets in motion the particles of the body, which almost at the same instant are briskly separated and left to the attraction of cohesion, that tends to unite them. Spheres of attraction are then established between the particles of

caloric

caloric that are flying off, and those of the substance itself; which tend to unite.

Metal gradually cooled

If this substance be a good conductor of heat, and the attractive power of its particles equal the expansive power of the caloric, it will return to its natural state without change of form. Such is a metal in fusion, which is left to cool gradually.

Cakes separated from cast iron.

If, under the same circumstances, the caloric, rapidly absorbed, is separated in successive strata, this substance will separate into planes, which will be perpendicular to the direction the caloric takes to escape. Such is the case with cast iron in fusion, the surface of which is wetted to separate thin cakes from it: a cause analogous to what may have divided basaltes into leaves, or thin strata, either perpendicular to the axes of the prisms, or around a globular mass.

Tempering of steel.

If, the motion of the caloric being uniform, the attraction of cohesion do not equal the expansile power, the particles of the substance will remain dilated; and then, if it be a good conductor of heat, they will maintain their situation without experiencing any division. Such is the effect of tempering steel, where the cohesive power of the particles of iron is broken by the interposition of carbon. But if the substance be not a good conductor of heat, it will fall to powder; as quartz strongly heated and immersed in water. The first of these circumstances has perhaps never occurred in volcanic productions, but the other must have been very frequent.

Quartz broken to powder.

Prismatic divisions.

To obtain prismatic divisions, let us suppose a basaltic mass still in its pasty state covering a considerable plain; and which, yielding to the laws of gravity, adheres strongly to the base that supports it. Then, if the expansive force of the igneous or aqueous gasses happen to cease in consequence of their sudden or accelerated extrication, the particles, losing their fluid state, will tend to approach each other, yielding to the laws of gravitation, and also obeying the attraction of cohesion that they exert toward each other; and they cannot contract, but by following the diagonal direction resulting from these two powers. But the extent of this mass, its gravity, and the inequality of surfaces, opposing a general contraction like that which is experienced by

by a cake of clay exposed to the fire on a support, there will necessarily be a vibration, and cracks that will determine spheres of attraction, round which the particles will agglomerate; and the centres will be so much more numerous, and the radii less, as the attractive force is more considerable.

VI.

Observations on the Oxigenized Muriatic Acid. By Mr. JOSEPH MOJON, *Professor of Pharmaceutic Chemistry in the Medical School of the Imperial University of Genoa, &c.**

IN making oxigenized muriatic acid, I have several times had occasion to observe, after having emptied the receiver, into which I had distilled the acid, and left it a few hours exposed to the light, that the little portion of acid, which commonly adheres to the inside of the receiver, lost entirely its peculiar suffocating smell, and acquired an aromatic odour perfectly analogous to that of muriatic ether. I remarked besides, that the oxigenized muriatic acid, though retained in bottles well stopped, and luted so that the gas cannot exhale, yet, if it remain some time exposed to the action of the sun, not only ceases to fume, but also acquires an ethereal smell, similar to that of muriatic alcohol or ether. Oximuriatic acid acquires the smell of ether:

This transmutation of oxigenized into simple muriatic acid, without the excess of oxigen being able to escape, as also the ethereal smell it acquires by simple exposure to light, led me more than once to suspect, that the oxigen in this case, instead of being extricated in the form of gas, entered into fresh combinations, and formed ether. changed into muriatic acid without any oxigen escaping.

To convince myself whether ether were really formed, I took a bottle filled with oxigenized muriatic acid, which had been left exposed to light almost two years, and had Ether obtained from some.

* Annales de Chimie, Vol. LXIV. p. 264.

acquired the ethereal smell. I have mentioned, I saturated it with magnesia; and distilled the whole in a glass retort with a very gentle heat, till I had obtained a few ounces of a fluid, which I rectified afresh in a small retort over a lamp. This afforded me a perfectly limpid, colourless liquor, of a very penetrating ethereal smell, and a taste resembling that of muriatic ether diluted with water. It did not change the colour of infusion of mallow flowers; and it did not take fire at the flame of a candle, being still very dilute.

Farther experiments promised.

The small quantity of liquor obtained by this process not allowing me to proceed to a fresh rectification, to deprive it entirely of the superabundant water it contained, I mean to make new trials with a larger quantity of acid.

From the observations I have thus briefly given, and which no doubt deserve to be repeated and confirmed by farther experiments, I am far from pretending to explain by vague hypotheses the formation of acohol, or of ether, by oximuriatic acid, and to point out whence it derives its component parts. We may suppose, however, that a portion of ether is formed at the time of distilling the oximuriatic acid, and that the potent and suffocating smell of this acid prevents that of the ether from being perceived. In fact the celebrated Giobert of Turin, in distilling oximuriatic acid sixteen years ago, observed a volatile oil similar to that which Mr. Westrumb had discovered some time before him. Mr. Giobert tells us, that this oil is of a yellowish brown colour, very clear, and analogous to the *oleum vini*; but that it is difficult to determine its precise quantity, since when once separated it dissolves anew very readily in the aqueous vapours, that fall into the receiver. This chemist imagined he might estimate the quantity of oil obtained from a mixture of a pound of sulphuric acid with eighteen ounces of muriate of soda at 30 or 35 grains.

Perhaps ether formed in the distillation,

as oleum vini found to be.

VII.

VII.

Extract of a Letter from Mr. Resal, *Apothecary at Remirement, to Mr.* Cadet, *Apothecary to the Emperor, on the Conversion of Malt Spirit into Vinegar, and on the Red Colour of Oil of Hempseed* *.

I TAKE the liberty of imparting to you an observation respecting the article of Mr. Hebert of Berlin, whose process you could not verify without it. I communicated it to Mr. Parmentier a twelvemonth ago, with several other notes, part of which was inserted in the month of May, 1806. One of these was on the vinegar of brandy, which chance threw in my way. I had mixed some malt spirit (*alcool de bierre*) with an equal quantity of water, and added to it some beech charcoal. Being set aside and forgotten, I was surprised at the end of a twelvemonth to find it converted into a very strong vinegar, and the unpleasant taste of the beer still subsisting. Malt spirit converted into vinegar.

With your permission I will add an observation respecting the property of liquids to absorb different solar rays.

It is known, that various substances absorb this or that luminous ray, but I do not believe that any one has mentioned the property, that oil of hempseed, *cannabis sativa L.*, has to absorb the red rays when they are direct only, and to appear of a fine blood-red colour; so that, being lighter than rape or linseed oil, as it returns to the upper part of the vessel it appears equally red, without changing the colour of the oil it floats on. Its use in the arts, since it offers more resistance to the air than linseed oil, and does not skin [*ne se crispe pas*] like it; and its mixture with oils for the lamp being very common from its low price, while it yields a thick smoke; require a method of detecting it. This that I have mentioned perhaps would answer, and even show the effect of the solar rays on different substances. Oil of hempseed grows red in the sun.

* Annales de Chimie, vol. LXIV, p. 261.

VIII.

Remarks on some Points of Hydrography, by Mr. LEBLANC, *Officer in the French Navy**.

Errour of longitude in the Gulf of Florida.

THE gulf of Florida, or new Bahama Channel, is greatly frequented by ships of all nations, that trade to or cruise in the Gulf of Mexico; yet the latitudes and longitudes of the principal points in it have not been fixed. They are not mentioned in the Tables inserted in our Connoissance des Temps, or in the English collection entitled "Tables requisite &c." Accordingly we are obliged to have recourse to the most modern charts. French navigators use the General Chart of the Atlantic Ocean published in 1791, and revised and corrected in 1792. I think I can show, that there exists an errour in longitude of 52′ with respect to all the points of the gulf. I was led to notice this on the following occasion.

corrected.

On the 25th of January, 1807, in the afternoon, on board the Foudroyant, we saw waves and breakers on the North of the Great Bahama. At 4 o'clock we set, at a small distance, Lena Key N 80° E, and that of Azena N 45° E by compass. The longitude given by our time-keepers No. 40 and 76, reduced to that hour, was only 80° 17′ 30″, while that by the chart was nearly 82° 15′. Whence it follows, that the whole course of the gulf is too far west about 52′ of a degree †, a considerable errour in those latitudes. The going and state of the two timekeepers had been carefully observed during our long stay at the Havannah. Their errours were almost nothing after we had been at sea eight days, when we had soundings abreast of Cape Henry. The results given by the observations taken with the reflecting circle gave us no reason to suspect any thing incorrect in the longitudes: and when we entered Brest the absolute errour of No. 40 was only 7′ of a degree after a voyage of thirty-five days.

Old Bahama channel.

Green Key is one of the principal marks of the Old Bahama

* Journal de Physique, vol. LXV, p. 55.

† I give the difference as in the original, not knowing where the errour is.

hama Channel. The English call it Chesterfield. There is a small errour in the latitude of this Key, as given in our Connoissance des Temps. In our voyage I ascertained it to be 22° 7′, instead of 21° 55′. The want of tolerable charts of this dangerous part, and the necessity of comparing the ship's place on the chart with sure data, render this observation interesting for those who sail without a pilot on board. As to the longitude, it was agreeable to what I obtained by the timekeepers. This key must not be confounded with another of the same name on the south of the Great Bank of Bahama, and almost in the same latitude.

The accuracy of both of the observations here given I have verified by comparison with two Spanish charts published in 1779 under the ministry of Mr. Langara, and derived from the Hydrographer's Office at the Havannah.

San Salvador in Brazil.

I know not where the latitude and longitude of San Salvador, one of the principal cities of Brazil, in the Bay of All Saints, are to be found. When we anchored in that bay, Mr. Fonsera, Captain in the Portuguese navy, and superintendant of that harbour, told me, that its latitude was 13°. and its longitude 42° 25′. An English work, in the hands of all the navigators of that country, gives them 12° 46′ and 41° 5′. So considerable a difference led me, to pay as much attention to the subject as our short stay would permit; and I had an opportunity of finding both by lunar observations and the timekeepers, that its true longitude is about 41° 5′. The latitude of point St. Antony I ascertained by several observations to be 12° 59·8′. The time of high water is twenty minutes after three, mean time. The variation of the needle there in 1806 was 10° 20′ E.

Variation of the needle.

IX.

On the Spontaneous Ignition of Charcoal: by B. G. SAGE, *Member of the Institute, Founder and Director of the first School of Mines*.*

Charcoal fired in grinding.

MR. de Caussigni appears to have been the first who observed, that charcoal was capable of being set on fire by the pressure of millstones.

* Journal de Physique, vol. LXV, p. 423.

Mr.

In fine powder ignites spontaneously.

Mr. Robin, commissary of the powder mills of Essonne, has given an account in the Annales de Chimie, No. 35, p. 93, of the spontaneous inflammation of charcoal from the black berry bearing alder, that took place the 23d of May, 1801, in the box of the bolter, into which it had been sifted. This charcoal, made two days be ore, had been ground in the mill without showing any signs of ignition. The coarse powder, that remained in the bolter, experienced no alteration. The light undulating flame, unextinguishable by water, that appeared on the surface of the sifted charcoal, was of the nature of inflammable gas, which is equally unextinguishable.

Moisture promotes this.

The moisture of the atmosphere, of which fresh made charcoal is very greedy, appears to me to have concurred in the developement of the inflammable gas, and the combustion of the charcoal.

In heaps heats strongly,

It has been observed, that charcoal powdered and laid in large heaps heats strongly.

and takes fire.

Alder charcoal has been seen to take fire in the warehouses, in which it has been stored.

About thirty years ago I saw the roof of one of the low wings of the Mint set on fire by the spontaneous combustion of a large quantity of charcoal, that had been laid in the garrets.

Fired in pounding.

Mr. Malet, commissary of gunpowder at Pontailler, near Dijon, has seen charcoal take fire under the pestle. He also found, that when pieces of saltpetre and brimstone were put into the charcoal mortar, the explosion took place between the fifth and sixth strokes of the pestle. The weight of the pestles is 80 pounds each, half of this belonging to the box of rounded bell metal, in which they terminate. The pestles are raised only one foot, and make 45 strokes in a minute.

Ingredients for gunpowder ground separately.

In consequence of the precaution now taken, to pound the charcoal, brimstone, and saltpetre separately, no explosions take place; and time is gained in the fabrication, since the paste is made in eight hours, that formerly required four and twenty.

Manufacture.

Every wooden mortar contains twenty pounds of the mixture, to which two pounds of water are added gradually,

The

The paste is first corned: it is then glazed, that is the corns are rounded, by subjecting them to the rotatory motion of a barrel, through which an axis passes: and lastly it is dried in the sun, or in a kind of stove.

Experience has shown, that brimstone is not essential to the preparation of gunpowder; but that which is made without it falls to powder in the air, and will not bear carriage. There is reason to believe, that the brimstone forms a coat on the surface of the powder, and prevents the charcoal from attracting the moisture of the air. Sulphur useful, not indispensable.

The goodness of the powder depends on the excellence of the charcoal; and there is but one mode of obtaining this in perfection, which is distillation in close vessels, as practised by the English. Goodness of charcoal important.

The charcoal of our powder manufactories is at present prepared in pots, where the wood receives the immediate action of the air, which occasions the charcoal to undergo a particular alteration.

X.

*Theory of the Detonation and Explosion of Gunpowder. By the same **.

THESE two phenomena, which take place simultaneously, arise from different causes. The detonation is the noise, that is produced by the combustion of two parts of inflammable and one of oxigen gas. Cause of the detonation of gunpowder,

The explosion, or discharge, is produced by the water of the nitre, and that which results from the decomposition of the two gasses, which, being expanded by the fire, occupies fourteen thousand times the space it did before; and acts in the same manner as compressed air, to which its elasticity is restored, and the explosive effect of which is produced without detonation. and its explosion.

The inflammation of gunpowder by means of a spark arises from the ignition of the nitre and brimstone. Its ignition.

* Ibid. p. 425.

The

Inflammable gas.

The inflammable gas is produced by the decomposition of the charcoal *; and the oxigen gas arises from part of the nitre, which is decomposed by the fire.

Foulness in gun barrels.

After the explosion of gunpowder, we find the inside of the gunbarrel coated with a mixture of alkaline sulphuret and charcoal not decomposed. This alkaline mixture attracts the moisture of the air, and forms a greasy coating within the barrel. If it be loaded in this state, part of the powder adheres to the sides of the barrel; and on discharging the piece, it catches, and produces what is termed *hanging fire*. The barrel of a fowling piece therefore should never be used a second day without cleaning.

XI.

On the Sulphates of Lime, Barytes, and Lead.

Mr. Thompson's analysis confirmed by Mr. Berthier

IN our last number, p. 174, we gave an analysis of two of these salts by Mr. James Thomson, who was led to the inquiry by the want of agreement between chemists respecting the proportions of the principles of the sulphate of barytes. A similar reason had led Mr. Berthier, mine engineer, to an investigation, which he has inserted in the *Journal des Mines,* for April, 1807, that has but lately reached this country. His analysis corroborates that of Mr. Thomson, after whose paper it would be superfluous to give Mr. Berthier's, I shall therefore simply quote the results he obtained.

Component parts of

"From the experiments I have above described it follows:

gypsum,

"1. That pure common gypsum, in whatever state of mechanical division it may be, contains 21 or 22 per cent of pure water.

Charcoal of hard woods best.

* In France charcoal of alder, poplar, willow, &c. is always used for making gunpowder. The intensity of the fire produced by such charcoal is less than of that from harder wood. The former, being more porous, would require more care in charring than the latter; and they cannot be said to be in the state of charcoal, unless they have been distilled: for when prepared by smothering the fire, there is always a portion reduced to the state of ashes [*braise*].

"2. That

"2. That the anhydrous sulphate of lime, whether natural or artificial, and the nonanhydrous sulphate calcined, contain the same proportions of lime and sulphuric acid; namely, 0·42 or 0·43 of lime, and 0·58 or 0·57 of acid, nearly as determined by Bergman. sulphate of lime,

"3. That the sulphate of barytes is composed of at least 0·33 sulphuric acid, and at most 0·67 of barytes. sulphate of barytes,

"4. That the mean proportions of these two salts are: 0·425 of lime, and 0·575 of acid, for the sulphate of lime, and 0·665 of barytes, and 0·335 of acid, for the sulphate of barytes. mean of both these,

"5. And lastly, that in pure calcined sulphate of lead there are 0·69 of metal, 0·26 of sulphuric acid, and 0·05 of oxigen." sulphate of lead,

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XII.

Extract from a Letter of Mr. Gehlen *to Mr.* Descotils, *on the Igneous Fusion of Barytes**.

It appears to me, that the French chemists are yet unacquainted with the fusibility of pure barytes by fire, which Mr. Bucholz discovered, and described in 1800, in the 2d number of his *Beitraege zur Erweiterung and Berichtigung des Chimie*. Igneous fusion of barytes

If pure barytes be heated in a platina or silver crucible, it liquefies in its water of crystallization. After this water is evaporated, it enters into fusion at a bright cherry red heat, and flows like an oil. On cooling, it becomes a gray mass, radiated in its fracture, which, when powdered, redissolves in water, heating more strongly than lime, and recrystallizes in cooling. succeeds the aqueous.

Mr. Bucholz, having hitherto prepared his pure barytes only in Pelletier's method, did not know by experience, that barytes did not melt when it has been prepared by the decomposition of the nitrate by fire; which it might have been expected to do, but which I have never seen take place, Does not take place with barytes obtained by decomposition of the nitrate.

* Annales de Chemie, Vol. LXIV. p. 168.

place, even with the strongest heat. Mr. Bucholz and I have made some experiments; to ascertain the cause of this; but we have not yet attained our object. Neither an excess of carbonic acid, nor the solution of part of the substance of the crucible, appears to be the occasion of this difference, since, on dissolving the residuum of the decomposition of the nitrate in water, very little insoluble matter remains in proportion to the quantity of barytes; and on adding this insoluble matter to pure barytes in much larger proportion the latter is not prevented from entering into fusion.

Perhaps previous crystallization necessary.

We know not whether the previous crystallization of barytes be necessary to the fusion, and consequently whether water do not act some part in it. This might be solved, by decomposing the nitrate in a crucible of some material not acted upon either by the nitrate or barytes. We made our experiment in a silver crucible, but obtained no decisive result, on account of the large quantity of silver, which the nitrate detached from the crucible by cohesion. As we have not a crucible of gold, or of platina, we cannot pursue our experiment. These observations, if inserted in your Annals, may perhaps tend to an elucidation of the subject.

Note by Mr. Descotils.

Proportions of the elements of the sulphate determined with fused barytes.

The French chemists have long known the igneous fusion of barytes, and it was with barytes thus fused, that Mr. Thenard determined the proportions of sulphate of barytes, which he gave in his Memoir on Antimony, published in 1800. It was likewise with fused barytes, that Mr. Berthollet has since determined the proportions of the principles of the same salt. As to the difference in fusibility of crystallized barytes and that which is obtained from the decomposition of the nitrate, Mr. Berthollet will make known the cause in a paper, which will be inserted in the 2d volume of the *Memoires d'Arcueil.* His experiments relating to barytes were already finished, when I received Mr. Gehlen's letter; and they had given occasion to a series of researches, that are now concluded. In Mr. Berthollet's paper it will appear, that water is the cause of the fusibility of barytes, as the two celebrated chemists of Erfurt have suspected; and that it

Water necessary to this fusion.

it is likewise the cause of the difference of the proportions of the principles of sulphate of barytes given by the chemists, who have attempted at different times to determine its composition.

XIII.

Note on a Species of Manna, or concrete Sugar, produced by the Rhododendron Ponticum.*

A Few years ago Messrs. Fourcroy and Vauquelin remarked, that a concrete sugar, or manna, exuded from the receptacle of the flowers of the pontic dwarf rosebay. Concrete sugar on the rosebay,

Mr. Bosc has lately observed it afresh, and presented to the Institute some grains of this substance collected by him from the receptacle of the fruit, several of which were upward of 2 mill. [0·79 of a line] in diameter. Their taste and appearance do not differ perceptibly from the purest sugar-candy; but it is necessary to be on our guard against this appearance, on account of the deleterious properties suspected in the plant. Mr. Deyeux has even found, that they leave an acerb smatch on the palate. described.

The manna of the rosebay, according to Mr. Bosc, is dissolved during the night by the moisture of the atmosphere, melted in the day by the heat of the sun, and does not exude from plants that vegetate vigorously. These are the reasons why it is so seldom seen. Plants growing in pots, and sheltered from the dew as well as from the sun, are most likely to furnish it. The grains above mentioned were collected from a plant, in which all these circumstances united. Reasons why seldom seen.

Mr. Bosc intends, if possible, to collect a sufficient quantity to analyse.

* Annales de Chimie, vol. LXIII, p. 102.

XIV.

XIV.

An Essay on Manures. By Arthur Young, *F. R. S.*

(Concluded from p. 196.)

7. *Yard and Stable Dung.*

Dung usually collected in heaps.

IT has been a common notion, till very lately, both with farmers and writers on agriculture, that dung is to be accumulated on hills or receptacles for a longer or shorter time, till fermentation and putrefaction have brought it, after few or many months, and few or many operations of turning or mixing, to a certain state, in which it is ready and proper for applying to land.

But it is sometimes used fresh:

But there is another system of management, which of late has attracted a good deal of attention: and this is, to use it fresh as made. If this method be right, no instructions for the management of dunghills are necessary, since we ought to have no dunghills.

and this is preferable, according to the ablest chemists,

Hassenfratz observes, "The management of the farmers "in Picardy is highly advantageous, in continually carrying "their dung to their land, rather than leaving it to be ex-"hausted in their farm yard, in order to be carried out at "a fixed period. By applying the dung quite fresh to the "land, its first fermentation is employed in heating the "soil. The little alkali it contains, instead of being dis-"solved in the farm yard, and carried off by rain, remains "in the land, and improves it, if alkali be useful to vege-"tation. The straw, yet entire, better divides the soil; its "fermentation proceeds less rapidly, and is less advanced "when the seed is sown; and consequently the dung is in "a better state for furnishing a great quantity of carbonic "acid, which hitherto appears to be, with water, the prin-"ciple aliment of plants."

Dr. Darwin asks a very interesting question. "Do the "recrements of vegetable and animal bodies, buried a few "inches beneath the soil, undergo the same decomposition, "as when laid on heaps in farm yards?" He conceives they do,

do, and adds: "Though this is accomplished more slowly, "yet it is attended with less loss of carbonic acid, of volatile alkali, of hidrogen, and of the fluid matter of heat; "all which are emitted in great quantity during the rapid "fermentations of large heaps of manure, and are wasted "in the atmosphere, or on unprolific ground. By using "dung in a less decomposed state, though it will require "some time before it will be perfectly decomposed and reduced to carbonic earth, it will in the end totally decay, "and give the same quantity of nutriment to the roots, "but more gradually applied."

The testimonies of Kirwan, Sennebier, and Dr. Pearson, are equally in favour of carrying dung fresh to the field.

What is still more to the purpose, the theory of these able chemists is supported by the authority of many of the most skilful and judicious farmers founded on extensive experiments. and the practice of the best farmers.

As dung is a compound of animal and vegetable matters, but chiefly the latter, it must be resolvable into the principles of which they are composed. Nature of dung.

These principles, thus separated by decomposition, will be ready again to enter into the composition of the growing vegetables. The grand property of dung therefore is, to yield immediate food to plants. Farther, it opens the soil, if this be strong; it attracts moisture; and by the fermentation, which it excites in the soil, promotes the decomposition of whatever vegetable particles may be already in the land. Its effects have powerful progressive influence; for the production of a great crop of leaf, root and stalk, by its shade and fermentation leaves the land in better order to produce succeeding crops. Its properties.

The circumstances to be considered in the receptacles of yard and stable dung are few but important. Collecting.

The first object to be attended to is to spread a layer of earth over the surface of the yard. Peat is the best for this purpose, with a portion of marle, or chalk. In want of this, turf, rich mould, scourings of ditches, and some marles, or chalk; but not so much of either as to prevent the penetration of the fluids, which should enter sufficiently, to give a black colour to the whole. There is no necessity for remov-

ing

ing this every time the dung is removed. As there are no advantages from fermentation in the mass till carried on to the land, no attention should be paid to prevent treading and pressing the mass. But as it is beneficial to have the whole as equal as possible, it is very useful, that the stable dung should be spread over the surface, and not left to accumulate at the door. The same observation is applicable to the riddance of the fat bullock stalls, and the hogsties. As heavy rains will at times, in spite of every precaution, cause some water to run from the yard, this should be received into a covered reservoir, and pumped up on heaps of earth prepared to receive it. In summer weeds of every kind, that do not propagate from the root, should be early collected and spread over the surface, as well as leaves in autumn; and the foddering with straw, if any, and the soiling on green food, should both be upon it for all loose cattle.

Preparation.

From what has been said it is obvious, that dung requires no preparation; but if the richness or quantity of the dung, or state of the weather, excite too much fermentation, or this be apprehended, scatter every now and then over the surface some of the same earth with which the yard was bedded, but not in layers.

State in which applied,

As soon as circumstances of crops and convenience will permit, the dung should be carried to the land. In a business of any extent this cannot be done exactly when the absorption of animal matter is enough to secure a due fermentation in the soil, but must be directed by other circumstances. The farmer however is not to lose sight of those principles, which govern the operation.

Application.

All dung should be applied to hoeing crops, to leys, or to grass land, and never to white corn. This is more essential with fresh long dung, than with short; as there will be many more seeds of weeds in it, several sorts of which are destroyed by a strong fermentation. The proper crops for which to apply yard and stable dung are turnips, cabbages, potatoes, beans, and tares for soiling; and the seasons for putting in these crops are spring, midsummer, and September. But the farmer is not confined to carry on his dung at the time of sowing or planting: it is, on the contrary, much better, especially with long dung, to have it previously deposited

deposited in the land. The dung made in the depth of winter may be spread in March or April for potatoes: the next made, and what is not wanted for potatoes, may be taken out in succession through April, May, and June, as convenience suits, for turnips and cabbages: that made in July and August will be ready for tares: and what is produced in September, October, and part of November, is ready for beans. The best time for manuring grass is immediately after hay is cleared from the field.

It is proper to remark, that the use of the skim coulter is essential to ploughing in long dung. By means of this admirable addition to any common plough, every atom may be buried*. Skim coulter.

8. *The Sheepfold.*

The immediate application of dung and urine to all soils, and of treading too loose ones, is well know to be productive of great benefit. Every one knows, sheep's dung and urine are so far from wanting fermentation previous to their being applied, that the sooner the seed is sown after folding, the greater is the effect: and this tends to confirm the principles laid down in the preceding section. Folding sheep.

9. *Pigeon's Dung.*

This manure is esteemed by farmers to be hot and powerful. Forty or fifty bushels per acre are commonly applied. While in the house it does not run into those stages of fermentation, that reduce a body to mucilage; and yet has an extraordinary effect when spread. This is another argument in favour of fresh dung. Pigeon's dung.

10. *Pond and River Mud.*

The quality of this must be affected by various circumstances. In proportion as it is resorted to by cattle and waterfowl, and receives the washing of towns, houses, farm yards, or privies, the mud must be good. In other cases the mud may be tried experimentally in small quantities, or chemically analyzed. It generally pays well, but seldom or never very considerably. Pond and river mud.

* See Journal, p. 52, on the utility of burying dung deep.

11. *Sea*

11. *Sea Weeds.**

Sea weeds.

Wherever these are to be had, they are used with uniform success. The best and most durable sort is cut from rocks at low water. One load used fresh is more service than two, that have been left in a heap to ferment. This is the case with nine substances out of ten.

12. *Pond and River Weeds.*

Pond and river weeds.

Great advantage has been found from cutting these weeds just before the last ploughing for turnips, and spreading them as a manure for that crop. Some value them load for load equal to dung, and have imagined the following barley superior to that after dunging for turnips.

13. *Hemp and Flax Water.*

Hemp and flax water.

In Yorkshire they observe, that the grass grows doubly where flax is grassed. Mr. Billingsley carted flax water on his land, and found it superior to animal urine. Where there are convenient ponds on a farm, one at least should be half filled in summer with green weeds for the putrid water, which would soon be the result.

14. *Burnt Vegetables.*

Burned vegetables.

In some parts of Lincolnshire it is usual, to spread evenly over land, just before sowing turnip seed, from three to 4 tuns of straw per acre, and set fire to it. A similar practice prevails in the Pyrenees. It is said to be superior to common dunging. In Cambridgeshire and other places very stout oat stubble, reaped high, is burned as a preparation for wheat, both cleaning and improving the land.

15. *Ploughing in Green Crops.*

Ploughing in green crops.

This husbandry has been practised for ages, though many have found little advantage from it. The benefit certainly depends on the crop being completely buried. The only way of proceeding is, to roll down the crop with a barley roller, and add a skimcoulter to the plough, going in the same direction as the roller, to plough six inches deep. There should be no other successive tillage than scuffling

* See Journal, p. 72.

shallow

shallow on the surface. It usually answers better for a summer's sowing, as of turnips, or early winter tares, than for late autumnal sowings.

General Remark.

On all arable farms the dung of the farm yard may manure from a sixth to a fourth of it; by a proper course of crops and layers a certain portion may be pared and burned; and at least one tenth may be manured by ploughing in green vegetables. By these three exertions a good manager may manure more than one third of his arable land every year, which, with a right application of calcareous manures, will keep any land in heart, and regularly in a state of improvement. General remark.

The preceding manures are usually to be procured on most farms. Under the second head, or such as are to be purchased, we have in the first class, or animal manures,

1. *Night Soil.*

This is the best of all manures, and, if dry, the cheapest. It answers on all soils, and for all crops; but the most profitable application of it is on grass lands, spread after clearing away the hay; though it may be used in all seasons. It is very durable in effect. The common quantity used is about 200 bushels an acre. In the state of powder it is excellent for delivering by drill cups with turnip seed. Night soil.

2. *Bones.*

These do best on strong soils, and their duration exceeds that of all other manures. The effect has been seen for above thirty years. For potatoes they are excellent. Five or six loads of fifty bushels each are commonly employed on an acre, after they have been broken and boiled for the grease. Bones, and

The refuse dust of bone manufacturers is also good. bonedust.

3. *Sheep's Trotters.*

These are a powerful manure, and usually sold by the quarter with feltmongers cuttings. Four or five quarters an acre are a common dressing, but eight have been spread. Sheep's trotters.

They should be ploughed in not less than 6 inches with a skim coulter.

4. *Hair.*

Hair.

Hog's hair is sold in great cities from 1s. to 1s. 6d. per bushel, pretty well squeezed down. From 16 to 25 bushels an acre are commonly used.

5. *Feathers.*

Feathers.

These are a powerful manure. Twenty-five bushels an acre have been spread with much success: but land, which unmanured yielded but 28 bushels of white wheat, with ten bushels of feathers produced 48.

6. *Fish.*

Fish.

Every sort of refuse fish is one of the most effective manures that can be carried into our fields.

7. *Graves.*

Graves.

These appear to produce remarkable effects in turnip crops on poor sandy soils.

8. *Woollen Rags.*

Woollen rags.

These do best on dry and sandy lands. From five to twelve hundred weight an acre, chopped small, are used.

Refuse of leather.

9. *Curriers Shavings and Furriers Clippings* do best on dry soils. Thirty bushels an acre are a common dressing.

10. *Horn Shavings.*

These are applicable to all soils, but do best in wet seasons. The coarser sorts are cheaper, but inferior in effect, though more durable.

Nature and properties of animal substances.

Nature & properties of animal substances.

All animal substances will fertilize the soil being resolved into their first principles, but this takes place much sooner with some, than with others. Urine begins to act immediately, bones will last twenty years. All of them should be laid on the field as soon as may be after collecting. Night soil, dry and in powder, is the only one properly applicable

plicable as a top dressing; the rest should be ploughed in as soon as spread.

In the second class Mr. Young includes

1. *Wood Ashes.*

Mr. Hassenfratz having questioned whether alkalis were a manure, Mr. Young made many experiments on the subject, which convinced him, that pearlash was in a very powerful degree; and that it also had the property of acting on charcoal by mere mixture and solution in water. Alkalis act on charcoal by mixture and solution in water.

Woodashes, wherever tried, have proved a valuable manure. Mr. Young has used them on gravel and loams, both dry and wet, and never without good effect. The spring is the proper season, and succeeding rain of much importance. Forty bushels an acre the common quantity. Woodashes.

2. *Peat Ashes.*

The value of these usually depends on the blackness and density of the peat that is burned. Those of the Newbury peat are most celebrated, and ten or twelve bushels an acre are a common quantity, while in other countries from twenty to forty are usually applied. According to Mr. Davy their component parts are Peat ashes.

Oxide of iron	48
Gypsum	32
Muriate of sulphur and of potash	20
	100

Some uncommonly ferruginous peat ashes are used with great success on the chalk hills of Dunstable.

3. *Coal Ashes.*

All sorts of ashes are found most effective when spread on clover, sainfoin, or other *seeds* in the spring. They are also good on grass lands, and are by many used on green wheat. The quantity from fifty to two hundred bushels an acre. The effect of fifty or sixty bushels on dry chalk lands is considerable. They answer best on dry, sound, rich loams; but on clays, and wet gravels or loams, they make a Coal ashes.

poor return. Coarse ashes and cinders are better than those that are finely sifted.

4. *Soot.*

Soot.

This is a very powerful manure on most soils; but least upon strong or wet clay. Twenty bushels an acre are the common quantity applied on green wheat or clover in the spring.

5. *Peat Dust.*

Peat dust.

From its abounding in hidrogen this should operate as a strong manure. Commonly too it contains much iron. Having a great attraction for humidity, it is very advantageous on dry sandy soils. Mr. Farey asserts it to be the best possible dressing for onions.

6. *Potash Waste.*

Potash waste.

The alkali having been extracted, this is not a powerful manure, but does good in low meadows, and on grass lands in general. Ten loads an acre, or 350 bushels, are a common quantity.

7. *Sugar-bakers Waste.*

Sugar bakers waste.

Some say this is a powerful manure.

8. *Tanners Bark.*

Tanners bark.

The tanning principle is probably in all cases hostile to vegetation. If this bark be ufeful any where, it should be on calcareous soils. Sometimes it appears to have diminished a crop of corn very considerably.

9. *Malt Dust.*

Malt dust.

Eighty bushels an acre have exceeded dung on clay land for wheat. From twenty to forty bushels are commonly used, and with success on various soils.

10. *Rape Cake.*

Rape cake.

About half a tun an acre is an excellent manure, but since the price has risen less is used. Mr. Coke, by drilling it in *powder* with turnip seed, makes a tun do for five or six acres.

Of

Of the fossil manures lime was included in the first division, and coal ashes were classed with those of wood and peat, so that only two remain.

1. *Salt.*

Little is known of this at present. In too large a quantity it is injurious. It is certainly beneficial when properly applied. Perhaps it is best when mixed with dung or compost. Salt.

2. *Gypsum.*

Many persons assert, that this is no manure; others, that it is almost uniformly advantageous. It is said, to act as an immediate manure to grass, and afterward in an equal degree to grain: to continue in force for several succeeding crops: to produce an increase of vegetation on stiff clay soil, but not sufficient to pay the expense: to be beneficial to flax on poor dry sandy land: to be particularly adapted to clover in all dry soils, or even on wet soils in a dry season: and to have no effect in the vicinity of the sea.

Of Composts.

These Mr. Young considers in the same light with dunghills: he is of opinion, that the materials composing them would produce at least equal if not superior effect when applied to the land directly. Composts.

XV.

On the Formation of the Winter Leaf Bud, and of Leaves. By Mrs. Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

YOUR obliging notice of my former papers has emboldened me, to trouble you again. There is no part of a plant or tree more various in its formation, and in its consequences more astonishing, than the *gemma,* or *bud.* In spite of Use of the bud not yet known.

of the abilities of a Malpighi, a Grew, and many others, its real use is not yet *perhaps* known. So defective were our magnifying glasses *at that time*, so impossible was it to render an opake object luminous and clear, that we cannot wonder they did not attempt to search farther into the formation of the bud: for there is hardly any study, that requires the objects being so much magnified, and opake specimens so clearly delineated. What follows I offer as the result of many years study; I offer it with the *greatest diffidence*, but with the most thorough conviction of its truth: nor have I trusted wholly to my own sight, many have seen the specimens on which I first founded my opinion, and drawn from them the same conclusions; which, though from their novelty they may surprise, will on farther examination in *very young* buds and leaves soon give conviction.

Method in which leaves are formed.

This opinion is, " That leaves are formed or woven by the " vessels or cotton, that is generally supposed by botanists " placed there to defend the bud from the severities of " winter. That these vessels are a continuation of those " of the bark and inner bark in the stem of the plant. " That these vessels compose the various interlacing " branches of the leaf, which are soon filled up by the con- " centrated and thickened juices of the inner bark, which " form the pabulum of the leaf."

This apparent on dissecting very early buds.

The truth of this assertion is easily seen by dissecting very early buds, where except two or three scales, nothing but these vessels will be found. What then could be the use of them?—to put them within *the bud* to keep the outside warm is against nature, for it is against reason. I shall begin with the anatomy of the bud from its first appearance; which will explain the whole process, as far as constant attention could give me an insight into it. The gemma or bud grows on the extremity of the young branches. It is a small round or pointed body; and is fixed on the young shoot, and along the branches on a sort of bracket. There are three sorts. The leaf bud, the flower bud; and the leaf and flower bud. It is the leaf bud alone I mean here to dissect: for their natures are totally different, as are the purposes for which they are intended. As I look on the leaf bud to be formed

Buds of three kinds.

almost

almost wholly of the bark and inner bark, so the flower bud is a composition of every *part* and *juice* of *the tree.*

The leaf bud.

The leaf bud is generally smaller than the other two; in its first state it consists of two or three scales, enclosing a parcel of vessels, which have the appearance of a coarse kind of cotton, very moist; but when drawn out, and placed in the solar microscope, they show themselves to be merely the vessels of the bark and inner bark elongated and curling up in various forms. They are generally of three sorts, like the bark, &c. First three or four short thick ones that appear to grow from the larger vessels of the inner bark, and through which the thickened juice flows, but with this difference, that the holes are not there. Then there are two smaller sized vessels, that exactly resemble the smaller vessels of the bark. The former I have ever found to be the midrib of the leaves; the latter the interlacing of the smaller vessels: and I have so often taken a leaf and dissected it to compare it with the vessels which I the next winter found in the leaf bud of the same tree, that I cannot but feel the most *thorough conviction,* that I have in *the bud* traced its origin; though certainly much enlarged in the full grown leaf. The pabulum of the leaf, or that which lies between the vessels, is (as I have before said,) composed of that thick juice which runs in the bark or inner bark of the tree, and is to be found in no other part. It differs essentially from the sap, and may be called the blood of the tree, as it possesses its peculiar virtues, is gum in one, resin in another, oil in a third, according to the nature of the plant. Whether it flows both *forward* and *retrograde* I have not yet been able to discover; indeed, finding the subject in the hands of a gentleman of such abilities as Mr. Knight, I waited his decision: but that the greatest part is taken up in *forming* the *leaves* I feel the most perfect conviction. The pabulum of the leaf, after the vessels are arranged and crossed, grows over in bladders, making alternate layers with the smaller pipes, and with the branches of the leaf. But I have found, and shall give, many specimens before this part of the process is begun.

Mr. Knight.

Formation of the leaf of the horsechestnut.

I know not any tree that gives a more convincing proof of the manner of forming leaves in the bud than the horse-chestnut:

chestnut: but it should be taken in November or December. Several different midribs may be taken at once from the same leaf bud, with an innumerable number of silken vessels extremely fine, fastened, or growing up each side the midrib. When these have interlaced each other sufficiently, the pabulum will begin to grow over them, in small bladders full of a watery juice. The next process is the larger vessels crossing over them, and then another row of bladders; this continuing till the leaf is at its proper thickness. The leaves thus formed are very small, but when once their shape is completed, they then continue growing all together.

A drawing will so much better explain this than any description, that I shall beg leave to refer to the sketch of the several specimens of beginning or half formed leaves taken out of the buds of various trees.

Mode of arranging the leaves in the bud.

When the leaves are so far completed, the rolling and folding begins. Each tree has its peculiar mode of arranging its leaves in the bud, as Linneus beautifully exemplifies, some double their leaves, and then roll them round one midrib; some round several, each of which has its own middle vessel; some plait, some fold the leaf. The variety is prodigious; but it must not be supposed, that once is sufficient to complete the *process*; I have had the most thorough conviction, that it is repeated several times, immersed all the while in the glutinous liquor, that runs in the bark, and forms the *pabulum*. During this arrangement, the pressure of the leaves is very great; and it is this and the rolling, that completes them; for if a leaf is taken from the bud, before this process, it will be like a piece of cloth before it is dressed; that is, with all the ends and knots to it; thus the back of the leaf will be obscured by the ends of vessels, which are at last all rubbed off, the hairs excepted, which remain to many plants.

Formation of the edge of the leaf.

The next process is the forming the edges of the leaves, the most curious and the most beautiful of all. The bud, if opened, will appear full of that glutinous liquor, and the leaves folded according to the order to which they belong. Take out one of them, and the edges, folded as it is, will exhibit a perfect double row of bubbles following the scollop of the leaf's edge, and appearing as if set with brilliants.

I hardly

I hardly know a more admirable spectacle in the microscope; it requires but trifling powers to show it well.

The last process, and completion of the leaf, is the forming of the pores. Whether it is, that the young leaf being thicker and more hairy than it is afterward, the pores are obscured and hidden, or that the upper net grows last, I cannot say; but in the many hundred forming leaves I have exposed to the solar microscope, I have never once been able to view the pores, as I have often done after the leaves had completely quitted the bud. I must not forget to mention, that there are two sorts of pores in the leaf; the large ones are those which receive the dew drops and rain, the smaller are those which appear in the day to give out the oxigen, and at night to inhale the carbonic gas. I mentioned, that I suspected these smaller pores of yielding a sort of insensible perspiration; as I find, when out of doors, a scurf only to be seen with a microscope; and under a glass this seems to rise as water, to bedew the glass. But to place an object in an unnatural situation, in order to judge of its secretions, is something like putting a human being into a warm bath, to judge how fast the blood flows. We know not what unnatural secretions we may cause in that confined air, or how much it may alter the nature of the plant, as I shall show at a future time with respect to melons and grapes.

Formation of the pores.

Two sorts of them.

Unnatural situations may occasion unnatural secretions.

The two cuticles of leaves differ in most plants: for in the under one I have hardly ever found the large pores into which the dew or rain enters; and but little oxigen is given out also from the under part of most leaves; while this part has a number of very small apertures, formed I suppose for the reception of the carbonic gas.

Upper and under cuticle.

I cannot but notice here how strange is the contradictory account of the leaves now generally received. They are supposed to perspire 17 times more than a man: water must therefore be yielded from each pore. They at the same time give out oxigen, and receive carbonic gas. Is this credible, or is it not contradictory? That they give out oxigen in the day, and inhale carbonic gas in *the night*, I am *convinced*, and I think it requires but the simple experiment of keeping a plant in the window, and examining it with a microscope 8 *or* 10 *times in a day*, to convince a person, that there

Contradiction in the received notions.

there is no perspiration worthy being so called. But I return to my subject.

Completion of the edge of the leaf.

While the upper and under cuticles are growing, the edge of the leaf is completing; the bubbles generally divide, and partly dry up, and horny points appear in their stead. When this is complete, the leaves burst from the bud; but there are few that will not show for a long time the manner of their formation; the planes for more than a month remain covered with the ends of vessels, some attached to the leaf, some loose: and most leaves have a bunch of vessels fastened at the outside to the corner of each side rib.

Two sorts of vessels.

The vessels of the leaves (I mean those confined within the midribs and side ribs of the leaf) *are of two sorts*; *the spiral* and *nourishing vessels.* The spiral vessel is that corkscrew wire, that surrounds the two last rows of the sap vessels (as I shall show when I describe the division into which the stem should, *I conceive* be separated). The nourishing vessels are the only part formed of the wood, and convey the sap necessary for the support of the leaf, and run on each side of the spiral ones; which are generally divided into little bundles of 3, 5, or 7 sets. It is impossible for any delineation to be more exact, than that given in the Phil.Trans. by Mr. Knight, of the entrance of these vessels into the midrib of the leaf. That these spirals vessels are the cause of motion in leaves, and that they are perfectly solid and incapable of carrying moisture, I hope to prove in my next letter.

Motive vessels.

Use of the hairs on leaves

Many leaves have a number of hairs fastened to the under cuticle of the leaf, and some to the upper. On the latter they appear designed to divide the rain drop to the size of the pore it is fitted for, and those at the back of the leaf seem intended to guard it from moisture, that the wet might not prevent the entrance of the carbonic gas at night; which it probably would do, without this precaution, by resting on the apertures. But it is watching nature in her natural state, that her laws are to be understood. When the wind blows with violence, the leaves turn their backs to the wind; and when the sun shines, they present their face to it: guarding by the first means the oxigen from *dispersing*, and in the latter case procuring a greater quantity, from the heat of the sun shining on the leaves. When the leaves

Why leaves turn from the wind & to the sun.

leaves are very young, they are pressed together, their backs exposed to the heat; probably to dry them, and clear the pores for the reception of the carbonic gas; and as young leaves give out hardly any oxigen, the shade in which the other side is immersed is of little consequence.

Description of the leaf.

To prove, that in forming the leaf I have given it no features, but what it really possesses, I shall finish by showing all the parts of a full grown leaf. The colour of leaves is not to be found in their substance, but in the liquid with which it is filled. The darkest green leaf that can be *taken*, has a perfect white cuticle, both *above* and *below it*. In this cuticle are the pores. It is rather a thicker net below than above; but not enough to account for the difference of the tints; but the under one lies not near so close to the pabulum of the leaf as the upper one; which may account for the colour not piercing so much through. When these two nets are taken off, the pabulum of the leaf appears. It is formed of little bladders, filled with a dark green liquid, and interlaced with vessels. Take this off, and a bed of larger vessels presents itself; then a collection of bladders, which is followed by the larger lines of the leaf; and then a bed of bladders repeated, which the under cuticle covers. Though the bladders differ in *size* and *colour* in different leaves, and in thickness also, yet the general arrangement is the same. I mean not however to include either the *firs*, the *grasses*, or those *grassy leaves* of early spring, the iris, crocus, snowdrop, &c., which are all of a different nature, as I shall show hereafter.

Different sorts of hairs.

I cannot quit the subject without adverting to the different sorts of hairs, that are found on the back and face of the leaf. I have before mentioned some on the former part, intended to preserve the dryness; but on the face of the leaf there appear often many filled with moisture, as a kind of reservoir for the cuticle, and these are replete or not, according to the dryness of the atmosphere.

Microscopic parasitical plants.

There is also an innumerable multitude of things, that are truly parasite plants, that grow on leaves, forming groves and orchards for the various tribes of insects, that live and breed under them. As I do not wish to mix the different subjects, I shall conclude this letter, but mean to trouble you with

with another on the division of the stem of plants, without which I cannot well explain the discoveries I think I have made with respect to the motion and general formation of plants, or the effect that grafting and budding of every kind have on trees; a study which is now occupying every moment of my time, and from which I hope to draw many useful hints.

The mistake made by my directing my letters to be sent to Mr. I. has led you into an errour. It is Mrs. Agnes Ibbetson, who has the honour of being your correspondent.

Dear Sir,

Your obliged servant,

Belleveu, June 8. A. IBBETSON.

Explanation of the Figures.

Explanation of the plate.

Plate VII, figs. 1, 2, 3, 4. Commencement of the growth of leaves, exhibited in different stages. *a, a, a, a,* the midrib, *b, b, b,* the young vessels appearing like cotton. *c, c,* the spiral nerves. *d,* the smaller vessels crossing each other.

Fig. 4. The formation of the pabulum. *e, e,* the fine vessels growing up each side of the midrib. *f,* the pabulum.

Fig. 5. Leaf-bud of the limetree.

Fig. 6. Leaf-bud of the horse chestnut about January.

Figs. 7, 8, and, 9, with some others, belong to two papers, which will appear next month.

XVI.

A Letter on a Canal in the Medulla Spinalis of some Quadrupeds. In a Letter from Mr. William Sewell, *to* Everard Home, *Esq. F.R.S.**

SIR,

Canal in the spinal marrow

According to your request, I send you an account of the facts I have ascertained, respecting a canal I disco-

* Philos. Trans. for 1809, Part I. p. 146.

vered

Fig 1.

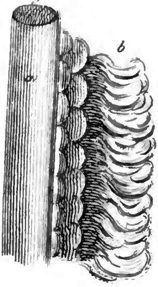


Fig 2.

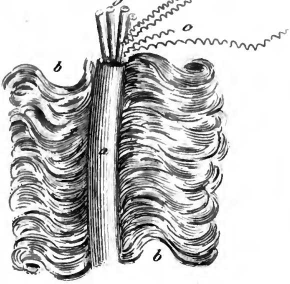


Fig 3.

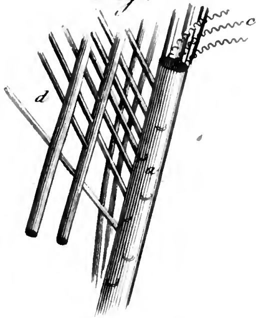


Fig 5.

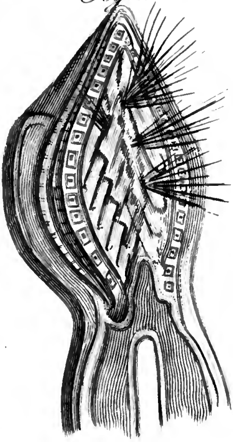


Fig 6.

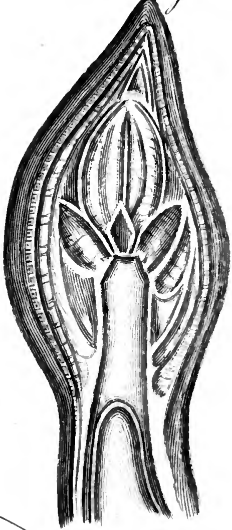


Fig 4.

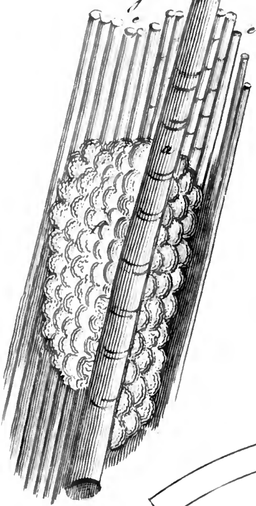


Fig 7.

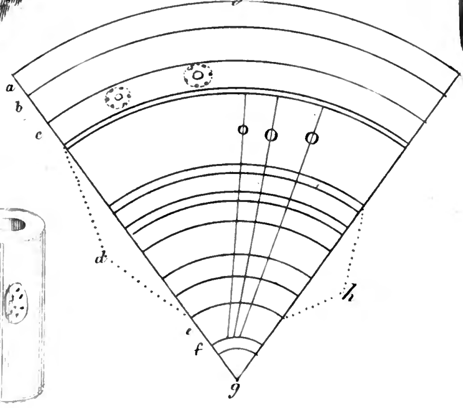


Fig 8.

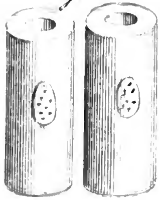
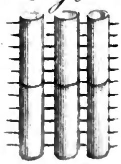


Fig 9.



vered in the year 1803, in the medulla spinalis of the horse, bullock, sheep, hog, and dog; and should it appear to you deserving of being laid before the Royal Society, I shall feel myself particularly obliged, by having such an honour conferred upon me.

Upon tracing the sixth ventricle of the brain, which corresponds to the fourth in the human subject, to its apparent termination, the calamus scriptorius, I perceived the appearance of a canal, continuing by a direct course into the centre of the spinal marrow. To ascertain with accuracy whether such structure existed throughout its whole length, I made sections of the spinal marrow at different distances from the brain, and found that each divided portion exhibited an orifice with a diameter sufficient to admit a large sized pin; from which a small quantity of transparent colourless fluid issued, like that contained in the ventricles of the brain. The canal is lined by a membrane resembling the tunica arachnoidea, and is situate above the fissure of the medulla, being separated by a medullary layer: it is most easily distinguished where the large nerves are given off in the bend of the neck and sacrum, imperceptibly terminating in the cauda equina.

communicating with one of the ventricles of the brain,

and containing a fluid.

Having satisfactorily ascertained its existence through the whole length of the spinal marrow, my next object was to discover whether it was a continued tube from one extremity to the other: this was most decidedly proved, by dividing the spinal marrow through the middle, and pouring mercury into the orifice where the canal was cut across, it passed in a small stream with equal facility towards the brain (into which it entered), or in a contrary direction to where the spinal marrow terminates.

A continuate tube the whole length of the spinal marrow.

By many similar experiments, I have since proved, that a free communication of the limpid fluid, which the canal contains, is kept up between the brain and whole extent of spinal marrow. I have consulted the most celebrated authors on comparative anatomy, but do not find any such structure of those parts described; and as it is not known to you, I may presume, that it has not been before taken notice of. I have the honour to be,

The fluid has a free communication with the brain.

Sir, your obedient faithful servant,

Veterinary College, Nov. 26, 1808. WM. SEWELL.

XVII.

Note on the Alteration, that Air and Water produce in Flesh. By Mr. C. L. Berthollet*.

Beef boiled in water repeatedly, exposed to the action of air,

I Boiled some beef, renewing the water from time to time, till the water no longer afforded a precipitate with tannin. I then suspended it in a glass cylinder filled with atmospheric air, which rested on a plate filled with water. After a few days the oxigen was found to be converted into carbonic acid: the interior of the cylinder was filled with a putrid smell: the beef, subjected to ebullition, again afforded a pretty copious precipitate with tannin: the boiling was repeated, till tannin ceased to render the water turbid: and the beef, having almost entirely lost its smell, was replaced in the same apparatus.

and again boiled.

This done repeatedly.

The operation was repeated several times, and the following were the results.

Results.

The alteration of the atmospherie air, and the emission of the putrid smell, gradually slackened: the quantity of gelatine formed progressively diminished: the water on which the vessel rested gave but slight indications of ammonia throughout the whole process: when I terminated it, no putrid smell was perceptible, but a smell resembling that of cheese: and in fact the animal substance, which scarcely retained any fibrous appearance, had not only the smell, but precisely the taste of old cheese.

Beef and cheese separately distilled.

I distilled separately equal weights of beef and Gruyere cheese, employing two glass bodies, each of which communicated with a tube opening under water. The operation was conducted so as to decompose the two substances as far as possible, and retain all the ammonia, that was evolved. On comparing the quantities of ammonia, that afforded by the cheese was to that of the beef nearly as 19 to 24: whence it appears, that a distinguishing characteristic of the caseous substance is to contain less nitrogen than flesh.

Less ammonia from the cheese.

* Journal de Physique, Vol. LXV, p. 466.

If

If any inference may be drawn from experiments so incomplete as the preceding, it would appear : Conclusion.

1. That the gelatine obtainable from an animal substance does not exist completely formed in it; but that, when this substance has been exhausted by the action of water, more may be formed by the action of the air, the oxigen of which combines with the carbon, while a portion of the substance, that was before solid, becomes gelatinous, as a solid part of a vegetable becomes solid by the action of the air. Gelatine not wholly formed in animal substances.

It must be remarked however, that the property of precipitating with tannin belongs to substances, that have very different qualities in other respects. I have found, that the decoction of Gruyere cheese formed a copious precipitate with tannin. Tannin affects different substances.

2. That nitrogen enters into the composition of the putrid gas, forming no doubt with hidrogen a combination less stable than ammonia, or perhaps taking an intermediate state; but, when its proportion is diminished to a certain degree, it is more strongly retained by the substance, and ceases to produce putrid gas. This substance, which is characterized by the putrid smell, appears to be rather a very evaporable compound, that unites with all gasses, like other elastic vapours, than a permanent gas. Putrid gas.

3. Since the caseous part has less nitrogen than most other animal substances, we may conjecture, that this part becomes more and more animalized during life, acquiring a greater proportion of nitrogen and hidrogen; which may be explained by the more intimate combination of the oxigen and hidrogen, that enter into its composition, and by the separation of carbon in the act of respiration; so that the last term of chemical action during life is the production of uree, agreeably to the opinion of Mr. Fourcroy*. Caseous matter. Uree.

* Syst. des Connoiss. Chim. tom. 10, p. 165; or English ed. Vol. X, p. 231.

XVIII.

XVIII.

Analysis of a Schist in the Environs of Cherbourg, taken from the Excavations made in Bonaparte Harbour. By Mr. BERTHIER, *Mine Engineer*.*

The rock described.

CONSIDERED separately, and in small masses, this rock has all the characters of the primitive formation. It is of a dirty green colour, and has the greasiness and lustre of talc, though in a very slight degree. Its texture is slaty, and a multitude of little grains of crystalline quartz, disseminated between its laminæ, are visible to the naked eye. Some have a laminated fracture, and are probably feldspar: we may unquestionably however consider it as of intermediate formation from its situation. In fact Mr. Descotils has observed, that it contains blocks of granite, frequently pretty large and rounded; and that it alternates with ancient breccias well characterized, talky and argillaceous schists, &c.

It would have been impossible to separate the quartz mixed with it, whatever pains were taken. Besides, the person who sent it to the laboratory desired, that it should be analysed as it was.

Analysis.

Five grammes [77 grs.] were fused with double their weight of caustic potash, dissolved in pure muriatic acid, evaporated to dryness, and the silex separated. The liquor being filtered, and tested with sulphuric acid and sulphuretted hidrogen, gave no precipitate. Hidrosulphuret of ammonia formed in it a black precipitate. Being filtered, oxalate of ammonia, afterward poured into the liquor, scarcely rendered it turbid; and potash precipitated a small quantity of magnesia. The sulphurets having been redissolved in nitromuriatic acid, the whole was precipitated afresh by saturated carbonate of potash. Nothing remained in the liquor, which proved the absence of manganese. Lastly the alumine and iron were separated by caustic potash.

* Journal des Mines, vol. XXI, p. 315.

The

The results of the different operations were

Silex	68
Alumine	15
Oxide of iron	5
Magnesia	2
Lime (at most)	1
	91

consequently there was a loss of 9 per cent. I knew indeed, that the whole of the silex was not collected; but what might be supposed to have been left was far from answering to this deficiency. Great deficiency.

Accordingly I took one hundred decig. of the substance reduced to powder, and calcined them strongly in a platina crucible. They lost 3 dec., and were slightly agglutinated. Six still remained to be accounted for, and, suspecting the presence of an alkali, I sought for it after Mr. Davy's method. Water expelled.

5 grs. were fused in a silver crucible with 10 of boracic acid. The whole was diluted in water; muriatic acid was added in excess; it was evaporated to dryness; an excess of acid was added afresh; and the silex was separated by filtration. The liquor, when sufficiently evaporated, deposited a great deal of boracic acid, which was removed. The whole was then precipitated by carbonate of ammonia, boiled, and filtered. The liquor, rendered again acid, and evaporated to a pellicle, deposited boracic acid, which was removed: and, the evaporation being continued, the residuum was calcined, to drive off the ammoniacal salts. What remained still contained boracic acid; and, whatever precautions were taken, it was impossible to separate it by evaporation. Hence this method, though very convenient for detecting the presence of an alkali, appears to me not well calculated for finding its proportion. Into the liquor, reduced to a few grammes, muriate of platina was poured, which occasioned a considerable precipitate, that was found to be the triple muriate of platina and potash, as will be related in the third analysis, that was made of the same schist. Second analysis. Not well adapted to ascertain the quantity of alkali.

 This

This was undertaken for the purpose of finding the quantity of the potash, the presence of which was certain, and of the silex, which had not been obtained with certainty.

Third analysis.

10 gram. [154 grs.] of the fossil were kept a long time at a red heat with five times their weight of caustic barytes. The mixture having grown pasty, it was diluted with water and pure muriatic acid. Being evaporated to dryness, the silex was collected. It weighed 7·05 gr. It was fused again with potash, and diluted in water and a little sulphuric acid. There was a residuum of 0·4 of a gr., to which muriate of silver gave a violet colour. It was heated red hot with carbonate of potash, and washed with distilled water. The liquor contained sulphuric acid. Great part of the residuum dissolved in muriatic acid. It contained barytes and silver. The 0·4 of a gr. therefore consisted of barytes, muriate of silver, and a little silex; so that we may reckon the whole of the silex at 7·1 gr.

The barytes was precipitated from the muriatic solution by sulphuric acid; the earths, and oxide of iron, by carbonate of ammonia. The filtered liquor having been evaporated to dryness, a residuum was obtained, which, being calcined with sulphuric acid, was reduced to 0·65 of a gr. It was redissolved in a very small quantity of water, and concentrated muriate of platina was added to the solution. A precipitate took place, which was collected. The supernatant liquor, decomposed by hidrosulphuret of ammonia, filtered, and evaporated afresh, left a residuum of 0·2 of a gr., consisting entirely of lime and magnesia. The least trace of soda was not to be found. There remained then 0·45 of sulphate of potash, containing about 0·25 of alkali.

Method of distinguishing the trisule of platina with potash from that with ammonia.

I satisfied myself, that the basis of this sulphate was potash by a very convenient method, which Mr. Descotils has made public, and which serves immediately to distinguish the triple muriate of platina and potash from that of platina and ammonia. It consists in boiling the precipitate in nitromuriatic acid. If it be the ammoniacal salt, it is decomposed, the ammonia is burned, and the platina dissolved. On the contrary, if it be the trisule with potash, it remains untouched; unless the quantity of the liquor be too great, in

in which case it dissolves, but reappears entirely by evaporation.

From the experiments that have been described it appears, that the schist analysed contains

Component parts of the schist.

Silex	71
Alumine	15
Oxide of iron	5
Magnesia	2
Lime (at most)	0·5
Potash	2·5
Water	3
	99
Loss	1
	100

It is possible, that the potash found in this schist comes from the feldspar, which I suspect to be in it. It would be interesting to ascertain, whether the alkali be inherent in the rock, by the analysis of a more homogeneous fragment.

The potash might be in the feldspar.

XIX.

Method of rendering common Alum as good for Dyeing as Roman Alum; by Mr. SEGUIN, *Corresponding Member of the Institute**.

TO the means that have been suggested for improving common alum, by freeing it from the iron it contains, Mr. Seguin has added a new one, founded on the different solubility of pure alum and alum contaminated with iron. He dissolves sixteen parts of common alum in twenty-four parts of water; crystallizes; and thus obtains fourteen parts of alum equal to the Roman, and two nearly equal to that of Liege.

Method of purifying alum.

This process might be employed in the manufacture of the alum, so as to obtain at first an alum worth one third more than in its impure state.

May be adopted in the manufacture.

* Sonnini's Bibliothèque Physico-économique, August 1807, p. 132.

SCIENTIFIC NEWS.

French National Institute.

French Institute.

MR. Delambre, perpetual secretary, has given an analysis of the labours of the mathematical division of the class of mathematical and physical sciences for the year 1807, of which the following is a brief account.

New construction of telescopes.

Mr. Burckhardt has proposed a mode of constructing telescopes, whieh he conceives will render their use more easy and convenient, than any yet adopted. His smaller mirror is plane, like Newton's, but placed perpendicular to the axis of the large concave mirror, and at half its focal distance. In this place the section of the reflected cone of light is a circle, the diameter of which is just half that of the large mirror. Accordingly the small mirror intercepts a fourth of the direct rays, but Mr. Burckhardt compensates this loss, by increasing the dimensions of the large mirror a little. The cone thus intercepted takes an inverted direction; and the rays, instead of proceeding to their focus behind the small mirror, unite at an equal distance in front of it, passing through an aperture in the centre of the large mirror. The telescope, thus reduced to half its length, will have four times as much light as a common reflecting telescope of the same length. Many objections were made to this construction, which Mr. B. answered, and it was agreed, that one should be made for trial.

Borda's circle.

The astronomers, who have lately measured the meridian line between Dunkirk and Barcelona, have employed Borda's circle to determine the time for correcting their clocks. They presume, that in an interval of four or six minutes, during which four or six observations may be taken, the altitude of the sun or a star increases with sufficient uniformity in proportion to the interval, so that a mean between the observations may be taken, and employed safely as a single observation.

Formulæ for altitudes.

Mr. Delambre and Mr. Burckhardt give several useful formulæ for taking altitudes of the stars, and likewise the moon,

moon, with precision. Mr. B. likewise proposes a new method of determining the moon's node.

Refraction of the atmosphere not affected by aqueous vapour.

Mr. Biot, before his first journey into Spain, had determined by nice experiments the refracting power of the air and of gasses, which he found to differ very little from what Mr. Delambre had inferred from his astronomical observations combined with those of Mr. Piazzi. It is well known, that refraction varies with the state and temperature of the atmosphere; and astronomers have long applied two corrections, one from the height of the barometer, the other from that of the thermometer. Since the introduction of the hygrometer, it has been questioned, whether this ought not to be employed for a third correction. During near a month, that Mr. Delambre spent in the steeple of Bois-commun, at a time when severe frosts more than once succeeded very damp mists, he endeavoured to ascertain, whether the variation of the hygrometer were attended with any change in terrestrial refraction, and found not the least indication of such a change. Mr. Laplace had made the important remark, that the refractive powers of air and the vapour of water, at equal degrees of elasticity, differed very little; but the question was of sufficient importance in astronomy, to be brought to the test of direct experiments. This Mr. Biot has undertaken. He first ascertained the effect of vapour alone. By means of potash he dried the warm air included in his prism, while that without was loaded with all the natural moisture of the atmosphere. The pressure of these two airs, indicated by a barometer within, and another without, was not the same; the difference being equal to the tension of the atmospheric vapour. The deviation of the luminous ray in the prism then gave the refraction produced by the vapour; and this never differed from what would have been produced by air alone at a similar temperature more than a few tenths of a second. The mean was 0·15″. Hence Mr. Biot infers, that the refraction produced by vapour in the atmosphere may safely be neglected in astronomy.

This doubted.

[Certain observations by some of the members of the Asiatic Society at Calcutta however lead to a different conclusion.]

Mr.

Nebula in Orion.

Mr. Messier has given a beautiful delineation of the nebula in Orion, to which he has added that of Legentil, and another much more difficult to perceive, which he himself discovered in 1773.

Violent storms.

He has likewise collected all the particulars of the thunder storm, that burst over Paris on the 21st of October, 1807; and the not less extraordinary gale of wind, that occurred the next day. In the observations he has registered for fifty years he finds nothing similar to it. The church of Moutivillers was struck by lightning during a storm equally violent, that took place on the 3d of November following.

Comet.

On the 21st of October Mr. Pons discovered the comet at Marseilles. It was then austral, near the horizon, and set soon after the sun. It was seen a few days after by different astronomers in France and Germany, and at Madrid. Mr. Burckhardt has calculated its orbit.

Other comets.

Mr. Burckhardt has found in the archives of the Imperial Observatory some unpublished observations of the comet of 1701, seen at Pau by Father Pallu. He suspects it is the same as was seen at sea in February following. Having found an important observation of the comet of 1672, he has calculated its elements afresh, and finds its perihelion distance greater than was before assigned; whence he infers, that it could not be the same with that of 1805, which some had supposed.

Tables of Jupiter and Saturn.

Mr. Bouvard has accomplished a more important and more generally useful task, corrections of the tables of Jupiter and Saturn; and Mr. Delambre has availed himself of these in the ecliptic tables of Jupiter's satellites, which he has entirely reconstructed, and will shortly publish.

Adhesion of water.

The only paper in physico-mathematics mentioned is Count Rumford's, printed in our Journal, Vol. XV, p. 52, from his communication.

Measure on the meridian.

Beside the Memoirs of the Institute, the second volume of the "Base of the Decimal System of Measures" has been published. It contains the remainder of the observations of all kinds, and the calculation of the triangles from Dunkirk to Barcelona; the heights of the signals above the surface of the two seas; the azimuths and the latitudes

latitudes of the five principal stations. The third and last volume is in the press.

Mr. Berthoud, who died in August 1807, had published a few days before his death a supplement to his treatise on Timekeepers, with an account of his researches from 1752 to 1807. Treatise on timekeepers.

Mr. Betancourt presented to the class a model of a lock on the same principle as that invented by Mr. Huddleston. [See Journal, Vol. IV, p. 236.] He has likewise given a mathematical discussion of the principles, on which it ought to be constructed, so as to be manageable by the strength of one man. Lock for canals.

Mr. Lancret has considerably extended Mr. Monge's theory of evolutes. Evolutes.

Mr. Malus, of the corps of engineers, has deduced from a uniform and general analysis the various circumstances of the propagation of light, and a solution of the fundamental problems of optics. By a theory entirely new, founded on the properties of the intersections of a series of right lines, drawn, according to a constant law, to all the points of a given surface, Mr. Malus has determined the course of refracted and reflected rays; the intensity of light, in all cases, at any given distance from the luminous point; and the place, form, and magnitude of images. He shows, that in certain cases, and with certain surfaces, reflection and refraction produce images, that are erect in one of their dimensions, and inverted in the other, a circumstance never before noticed*. Propagation of light.

The propagation and reflection of sound have some re- Propagation

* The plane mirror, or common looking-glass, in fact shows objects erect in the perpendicular, and inverted with respect to right and left. But this is not what the reporter means, though he does not inform us, what the construction of the mirror of Mr. Malus is. It would be found however, that a mirror, which is a section of a concave cylinder, will represent the horizontal dimension of an object the reverse of what a plane mirror would do, without affecting the perpendicular; in other words, the spectator would see the image of himself, or any other object in it, exactly in the same position, as if he stood facing the object, that occasioned the image: and this no doubt is the mirror alluded to, which is of a kind, that I do not recollect to have seen mentioned. C. Mirror of a new kind.

semblance

and reflection of sound.

semblance to those of light, but their theory is attended with more difficulty. As the velocity of sound is very small, it might be questioned how far it depended on a simple law. Messrs. Lagrange and Euler, who first treated this problem, supposed it in a particular case to depend only on its distance from the centre of motion. Mr. Poisson has just demonstrated generally, in a very ingenious manner, that the law is always the same: that the movement is propagated by spherical undulations with the same velocity in every direction; but that the vibration of particles situate at the same moment in the sonorous wave are made with unequal rapidity, according to a law depending on the nature of the primary agitation; and consequently, that the intensity of the sound, which depends on the velocity of these vibrations, is thus found to be different in different parts of the sonorous wave. The velocity in a given radius decreases in the ratio of the distance; whence it follows, if the intensity be proportional to the square of the velocity, it must decrease in the proportion of the square of the distance.

Only two determinate roots of the general equation had been found, but the formulæ of Mr. Poisson comprise an infinite number, by which may be verified all the theorems he has obtained in the general case, to which he first paid attention. He afterward considers the case where there are several causes of a simultaneous vibration; and without affecting the generality of the root, he decomposes it so, that the different parts answer to the different centres; which leads him to give in a novel and ingenious manner the theory of the reflexion of sound, and production of echoes; and to show what would take place between opposite and parallel planes. By a similar method he explains what must occur in the far more difficult case, where the mass of air set in motion is included in an ellipsoid. He demonstrates, that the sound, which originates in one of the foci, is reflected toward the other, making the angle of reflection equal to that of incidence, and following the same laws as light. These results are conformable with what we have learned by experience of elliptical vaults, but it was very difficult to demonstrate them mathematically, which Mr. Poisson has done in a new and ingenious manner.

It

It has long been remarked, that the observed velocity of sound is superior to what is deduced from algebraical calculations. It may be conceived, that the density and temperature of the air have some influence in this; but Mr. Poisson demonstrates, that they are insufficient to explain the observations. Having examined successively the causes supposed by Newton and other geometricians, he finds them incompatible with the results of sound philosophy. Mr. Laplace attributes the acceleration of sound to the change of temperature experienced by the particles of air in their condensation and dilatation, which cannot take place without a successive evolution and absorption of heat. Calculation applied to this hypothesis, or rather incontestable fact, shows, from experiments made by the Academy of Sciences in 1738, that a dilatation or condensation of $\frac{1}{116}$ produces a change of temperature equal to a degree of the centesimal thermometer [1·8 Fah.]. Velocity of sound.

The labours of the physical division of the class have been analysed by Mr. Cuvier, perpetual secretary.

In 1804 the class had awarded a prize to Doctors Herholdt and Rafn, of Copenhagen, for a paper on the winter sleep of animals; and, in 1807, another to Dr. Saissy, of Lyons. Prof. Prunelle, of Montpellier, has since sent a paper, that may rank with the best on the subject. Still however, notwithstanding their researches, and those of Spallanzani, Mangili, and Carlisle, we are ignorant of the causes, by which certain animals are disposed to this sleep, and not others; as well as of those that enable them to endure this suspension of their functions. Hibernation of animals.

Mr. Geoffroy-Saint-Hilaire, Prof. at the Museum of Natural History, elected to succeed the late Mr. Broussonnet, presented to the class some fragments of a great work, which he has undertaken on comparative osteology. His object is to investigate more minutely the analogies between the corresponding parts of various animals with vertebræ. In fact those parts of organs, that are always found more or less similar in number and position, notwithstanding their difference in size and use, and contradictoriness to all apparent final causes, must necessarily depend on efficient and formative causes. As these must be connected with the primary Comparative osteology.

means

means employed by nature, if we may flatter ourselves with ever throwing any light on the origin of organized bodies, the most obscure and mysterious point of natural history, it seems to us the first sparks must be derived from these analogies of structure.

Mechanism of respiration in fishes.

Mr. Dumeril, prof. of anatomy at the Medical School, presented three papers. In the first he treated on the mechanism of respiration in fishes, and pointed out some interesting singularities. Those that from having their mouths sometimes affixed to stones, or buried in mud or sand, cannot always use them for taking in water, are provided with apertures for admitting the water on dilating the cavity of the mouth, and these apertures are furnished with valves internally, to prevent the water from returning by them, so that it has no exit but by the gills.

Organ of taste in fishes.

The second was on the smell and taste of fishes. Mr. D. supposes, that the tongue, from the dryness and hardness of its integuments, and the constant passage of water over it, must be insensible to flavours; and that the pituitary membrane, not being exposed to the impulse of elastic vapour, cannot be the seat of smell like ours. This membrane therefore he conceives to be the organ of taste.

Reptiles,

The third is a comparison of the various vital and animal functions in the order of reptiles termed *batrachian*, which justifies its division into two families.

Crocodiles.

Several other papers on reptiles have been produced, particularly on crocodiles, of which Mr. Cuvier has shown no less than twelve distinct species exist in the old and new world.

Amphibia.

The same naturalist has endeavoured to remove by dissection the doubts entertained respecting some reptiles of a singular form, which truly deserve the name of amphibia, because they breathe both with gills and lungs. One of these is the siren lacertina, another the proteus anguinus*, and a third the proteus pisciformis. The two former of these at least have the skeleton too firmly ossified, and too different from those of any other reptile of their native abodes, and besides their organs are too perfect, to admit of their being

* See Journal, Vol. XVIII, p. 91.

considered

considered as tadpoles, that have a change to undergo. The last inhabits the lakes of Mexico, where it is used as food, and resembles a water lizard, except in having gills. It is called there *axolotl*, and was brought over by Humboldt. Axolotl.

Mr. Biot, while employed in measuring an arc of the meridian at the Balearic Islands, thinks he has observed, that part of the intestines of fishes caught by a hook and line at great depths, and drawn up suddenly, issue out of their mouths, which he attributes to the expansion of the air-bladder. He has likewise examined the nature of the air in this bladder, and found it to vary from pure nitrogen to a mixture of this gas with 0·87 oxigen, but he discovered no hidrogen. It appeared to him, that, the deeper the fish lived under water, the more oxigen the air contained. Air-bladders of fishes.

Mr. Jurine is extending his new method of classing insects*, which is found to be more natural than could have been expected, to the diptera. Entomology.

Mr. Dupuytren, head of the anatomical department of the Medical School, has shown, that the concurrence of the nerves of the lungs in the act of respiration is necessary to the conversion of the venous blood into arterial. Nerves of the lungs necessary in breathing.

The science of botany has been sedulously pursued. Mr. de Labillardiere has finished his Flora of New Holland. Mr. Dupetit-Thouars continues his researches on the growth of vegetables. He still thinks, that the trunk of trees has the principle of its increase in the buds; and that the fibres composing the annual layers of wood are in some sort the roots of the buds, while the little medullary thread terminating each bud performs the functions of cotyledons. He has endeavoured to answer objections, and brought forward many interesting facts. Among these is the germination of the *lecythis*. The evolution of the seed of this tree, which is dicotyledonous, cannot be referred to either of the three modes hitherto adopted. Its cotyledon is interior, and serves as a base to the pith, which Mr. D.T. thinks a proof of the justice of his opinion. The cuttings of the Growth of vegetables.

* See Journal, Vol XVIII. p. 218.

willow,

willow, that take root though deprived of their buds, seem to furnish a strong objection to it; but he has found, that in this case little subsidiary buds are unfolded opposite points that were occupied by the stipules of the leaves.

Carbon of plants.

There is no subject of more general importance in the vegetable economy than the origin of the carbon of plants. Mr. Crell, the celebrated chemist of Helmstadt, has this year communicated to the class some experiments, that seem to give a very high notion of the power of vegetation. He asserts, that he has made plants grow and produce seed in pure sand, watering them only with distilled water, and supplying them with a given quantity of air, in which the carbonic acid must be almost as nothing in proportion to the carbon produced. It is to be observed however, that, though the plants were covered with a glass, he could not prevent the access of the external air through the sand.

Philosophical and chemical Society of Arcueil.

Messrs. Laplace, C. L. Berthollet, Biot, Gay Lussac, von Humboldt, Thenard, Decandolle, Collet-Descotils, and A. B. Berthollet, have formed a society under the name of Philosophical and Chemical at the village of Arcueil, near Paris, which meets once a fortnight, and published the first vol. of its Memoirs in 1807.

Berlin Society.

At the Royal Academy of Sciences at Berlin, the sixth of August last, a paper on the resistance of the air was read by Mr. Burja; one on the advantages and disadvantages of national prejudices by Mr. Klein; and a fragment on the great cataracts of the river Oronoko by Mr. von Humboldt. The following prize subject is proposed for 1810. "To "give a complete theory of the hydraulic ram, paying regard "to the adhesion of water*."

Prize question.

Dr. Gauss has sent to the Royal Society of Gottingen the following observations of two of the new planets.

Observations of Pallas.

1st. *Observations of Pallas.*

1806.	Mean time. *hours.*	Apparent right ascens.	Apparent declination.
Feb. 14.	8 11′ 16″	70° 16′ 31″	19° 59′ 13″ S.
16.	7 32 28	70 42 39	19 20 44
17.	6 52 38	70 56 44	19 1 8
20.	7 49 35	71 39 2	18 5 0

* For a description of its mechanism, and some remarks on it, see Journal, vol. XIV, p. 98.

2d. *Obser-*

2d. *Observations of Juno.*

1806.	Mean time. hrs.	Apparent right ascens.	Apparent declin.
Feb. 17.	9 42′ 0″	173° 46′ 45″	0° 28′ 32″ N.
20.	10 49 47		0 54 18
	10 59 2	173 15 57	
	13 12 18	173 15 15	

Of Juno.

The following observations of Juno were made at Gottingen.

1806.	Mean time. hrs.	Apparent right ascension.	Apt. declin.
March 10	9 53′ 56·3″	169° 46′ 54·5″	3° 41′ 50·5″
11.	10 32 22·7	169 34 18	3 51 56·5

Dr. Gauss has likewise sent new elements of the orbit of Ceres, deduced from the last opposition observed by prof. Pasquich, which the doctor means to render more correct, when he has observations of this opposition on which he can better rely.

Elements of Ceres.

Epoch of the longitude, meridian of Seeberg..........	108°	19′	34·7″
Diurnal tropical motion	770″	85′	84
Annual	78	9	23
Aphelion, 1806	326	37	59
Annual motion	+	2	1·2
Ascending node, 1806......	80	53	23
Annual motion............	+		1·5
Inclination of the orbit, 1806	10	37	34
Annual diminution			0·4
Eccentricity, 1806	0·0783486		
Annual diminution	0·0000058		
Log. of the greater semiaxis..	0·4420728		

To the observations of Vesta, given in our Journal, vol. XVIII, p. 75, we can now add the following.

Observations of Vesta.

1807.	Mean time. hrs.	Apparent right ascension.	Apparent declin.
April 1.	9 50′	183° 28′	12° 5′ N.
5.	11 17 2·784″	182 33 10·92	12 24 19·1″
6.	11 12 16·022	182 20 47·91	12 27 54·4

The

The first of these is by Dr. Olbers, the other two from the observatory of Gottingen.

Dr. Gauss has determined its elements in the following manner.

Elements of Vesta.

Epoch of the mean longitude at Bremen, March 29, 1807, at 12 o'clock, mean time	193°	8′	4·6″*
Longitude of its perihelion	249	7	41
——— aphelion	69	57	52
——— ascending node on the ecliptic	103	8	36
Inclination of its orbit	7	5	49·5 †
Diurnal tropical motion	0	16	18·91
Logarithm of the mean distance	0·3728428		
Eccentricity	0·097505		
Greatest distance from the sun	25·625		
Least	21·514		
Period of its revolution	1321 days, 12 hours.		

Mathematical part of Humboldt & Bonpland's travels.

The fourth part of von Humboldt and Bonpland's Travels will contain in two 4to. vols. the astronomical observations, trigonometrical operations, and barometrical measures. Mr. von H. has thought it would be most satisfactory to give the whole of the original observations themselves, that it may be seen what degree of confidence the results deduced from them deserve. The calculations have been made by Mr. Jabbo Oltmanns from the best tables. The magnetical observations, with an examination of them and of those of Cook, Vancouver, and other able astronomers, by Biot, will occupy the 2d. vol. As such a number of figures must be a long while printing, the latitudes and longitudes of various places, deduced from astronomical observations, have been published in a separate tract in Latin.

Statistical account of Mexico.

In the third part of their travels, consisting of a statistical Essay on the Kingdom of New Spain, they estimate the present population of Mexico at more than six millions.

* In the Magazin Encyclopédique it is 192° 9′ 54″.

† Ibid. 7° 8′ 34′.

They

They likewise give the following comparative table of births and deaths.

	BIRTHS.	DEATHS.
In France	110	100
In England	120	100
In Sweden	130	100
In Finland	160	100
In the Russian Empire	166	100
In Western Prussia	180	100
In the government of Tobolsk	210	100
In several parts of the high plains of Mexico	230	100
In the state of New Jersey, North America	300	100

Table of mortality in various places.

Famine however not unfrequently interferes, to check the population of Mexico. In 1784 no less than 300000 died for want. The mortality among the miners does not appear to be greater than in other classes. The heat of most of these mines is very considerable. At the bottom of that of Valenciana, at the depth of 513 met. [560 yards] the centigrade thermometer was at 34° [93·2° Fahr.], while in the open air in winter it is only 4° or 5° above 0 [from 39·2° to 41° F.].

Famine.

Miners not unhealthy.

Heat of the mines.

On the 22d of August last Mr. Andreoli and Mr. Brioschi went up with a balloon at Padua. When the mercury had fallen to 15 inches [about the height of $3\frac{1}{2}$ miles] Mr. B. began to feel an extraordinary palpitation of the heart, without any painful sensation in breathing. When the mercury was down to 12 [$4\frac{1}{2}$ miles] he was overpowered with a pleasing sleep, that soon became a real lethargy. The balloon continued ascending, and when the mercury was about 9 inches [near 6 miles] Mr. A. perceived himself swollen all over, and could not move his left hand. When the mercury had fallen to 8·5 [about 6 miles and a quarter high] the balloon burst with a loud explosion, began to descend rapidly with much noise, and Mr. B. awoke. It fell about 12 miles from Padua, without any injury being received by the aerial travellers.

Ascent with a balloon to a great height.

The scheme of bishop Wilkins I understand has been pursued with some success at Vienna. A watchmaker of the name of Degen is reported to have ascended above the trees in the Prater with artificial wings, taken his flight in various directions, and alighted on the ground with as much ease as a bird.

Artificial wings.

Meteorolo-

METEOROLOGICAL JOURNAL,

For JULY, 1809,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JUNE Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
26	57	58	65	51	30·42	Fair	Cloudy
27	58	59	67	52	30·22	Ditto	Fair
28	57	56	62	50	30·08	Rain	Cloudy
29	56	57	61	51	30·01	Ditto	Rain
30	57	56	62	54	29·95	Ditto	Fair
JULY							
1	56	63	67	58	29·83	Fair	Ditto
2	63	58	62	51	29·79	Rain *	Rain
3	53	51	63	49	29·53	Ditto	Ditto
4	49	52	55	49	29·48	Ditto†	Cloudy
5	52	53	55	53	29·52	Ditto	Ditto
6	53	58	60	57	29·70	Ditto‡	Ditto
7	62	61	65	60	29·81	Ditto	Ditto‖
8	61	59	66	52	29·82	Ditto	Rain
9	53	52	56	52	29·88	Ditto §	Cloudy
10	52	53	57	50	29·88	Ditto	Fair
11	53	54	64	58	30·03	Fair	Ditto
12	61	65	68	62	30·09	Ditto	Ditto
13	62	62	68	55	30·09	Ditto	Ditto
14	62	62	69	60	30·18	Ditto	Ditto
15	63	63	68	60	30·09	Ditto	Ditto
16	63	64	72	59	30·00	Ditto	Ditto
17	62	59	67	53	29·76	Ditto	Ditto
18	58	55	60	50	29·90	Ditto	Ditto
19	56	62	65	57	30·03	Ditto	Ditto
20	60	62	66	55	30·12	Ditto	Ditto
21	60	61	65	55	30·20	Ditto	Ditto
22	61	62	63	56	30·05	Ditto	Ditto
23	59	61	69	60	29·89	Ditto	Ditto
24	60	61	69	60	29·86	Ditto	Cloudy
25	62	65	74	61	29·78	Ditto	Ditto¶

* A.M. at 1 P.M. thunder and lightning the thermometer retiring 2°.
† Hail, thunder and lightning at 2 P.M. the thermometer retiring 4°.
‡ Rain the whole day.
‖ At 11, lightning, thunder, and heavy rain.
§ Rain the whole day.
¶ Heavy rain, thunder, and lightning in the night.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXIII.

ARTICLE I.

The Bakerian Lecture. An Account of some New analytical Researches on the Nature of certain Bodies, &c. By HUMPHRY DAVY, ESQ. *Sec. R. S. F. R. S. Ed. and M. R. I. A.*

(Continued from Page 257.)

3. *Analytical Experiments on Sulphur.*

Sulphur seemed to contain hidrogen.

I HAVE referred, on a former occasion*, to the experiments of Mr. Clayfield and of Mr. Berthollet jun., which seemed to show that sulphur, in its common form, contained hidrogen. In considering the analytical powers of the voltaic apparatus, it occurred to me, that though sulphur, from its being a nonconductor, could not be expected to yield its elements to the electrical attractions and repulsions of the opposite surfaces, yet that the intense heat connected with the contact of these surfaces might possibly effect some alteration in it, and tend to separate any elastic matter it might contain.

Experiments to ascertain this.

On this idea some experiments were instituted in 1807. A curved glass tube, having a platina wire hermetically sealed in its upper extremity, was filled with sulphur. [See our last Number, Pl. VII, Fig. 4.] The sulphur was melted

* Bakerian Lecture, 1808, p. 16; or Journal, Vol. xx, p. 302.

over a spirit lamp; and a próper connection being made with the voltaic apparatus of one hundred plates of six inches, in great activity, a contact was made in the sulphur by means of another platina wire. A most brilliant spark, which appeared orange coloured through the sulphur, was produced, and a minute portion of elastic fluid rose to the upper extremity of the tube. By a continuation of the process for nearly an hour, a globule equal to about the tenth of an inch in diameter was obtained, which, when examined, was found to be sulphuretted hidrogen.

Sulphuretted hidrogen produced.

But the sulphur might have contained water.

This result perfectly coincided with those which have been just mentioned; but as the sulphur that I had used was merely in its common state, and as the ingenious experiments of Dr. Thompson have shown, that sulphur in certain forms may contain water, I did not venture, at that time, to form any conclusion upon the subject.

The experiment repeated with pure sulphur.

In the summer of the present year, I repeated the experiment with every precaution. The sulphur that I employed was Sicilian sulphur, that had been recently sublimed in a retort filled with nitrogen gas, and that had been kept hot till the moment that it was used. The power applied was that of the battery of five hundred double plates of six inches, highly charged. In this case the action was most intense, the heat strong, and the light extremely brilliant; the sulphur soon entered into ebullition, elastic matter was formed in great quantities, much of which was permanent; and the sulphur, from being of a pure yellow, became of a deep red brown tint.

Sulphuretted hidrogen produced, and part of the sulphur acidified? Large quantity evolved.

The gas, as in the former instance, proved to be sulphuretted hidrogen. The platina wires were considerably acted upon; the sulphur, at its point of contact with them, had obtained the power of reddening moistened limtus paper.

I endeavoured to ascertain the quantity of sulphuretted hidrogen evolved in this way from a given quantity of sulphur, and for this purpose, I electrized a quantity equal to about two hundred grains in an apparatus of the kind I have just described, and when the upper part of the tube was full of gas, I suffered it to pass into the atmosphere; so as to enable me to repeat the process.

When

When I operated in this way, there seemed to be no limit to the generation of elastic fluid, and in about two hours a quantity had been evolved, which amounted to more than five times the volume of the sulphur employed. From the circumstances of the experiment, the last portion only could be examined, and this proved to be sulphuretted hidrogen. Towards the end of the process, the sulphur became extremely difficult of fusion, and almost opaque, and when cooled and broken, was found of a dirty brown colour.

Sulphur and potassium evolve sulphuretted hidrogen.

The experiments upon the union of sulphur and potassium, which I laid before the Society last year, prove that these bodies act upon each other with great energy, and that sulphuretted hidrogen is evolved in the process, with intense heat and light.

Potassium heated with compound inflammables.

In heating potassium in contact with compound inflammable substances, such as resin, wax, camphor, and fixed oils, in close vessels out of the contact of the air, I found, that a violent inflammation was occasioned; that hidrocarbonate was evolved; and that when the compound was not in great excess, a substance was formed, spontaneously inflammable at common temperatures, the combustible materials of which were charcoal and potassium.

Pyrophorus.

Analogies.

Here was a strong analogy between the action of these bodies and sulphur on potassium. Their physical properties likewise resemble those of sulphur; for they agree in being nonconductors, whether fluid or solid, in being transparent when fluid, and semitransparent when solid, and highly refractive; their affections by electricity are likewise similar to those of sulphur; for the oily bodies give out hidrocarbonate by the agency of the voltaic spark, and become brown, as if from the deposition of carbonaceous matter.

Hidrogen certainly exists in sulphur.

But the resinous and oily substances are compounds of a small quantity of hidrogen and oxigen, with a large quantity of a carbonaceous basis. The existence of hidrogen in sulphur is fully proved, and we have no right to consider a substance, which can be produced from it in such large quantities, merely as an accidental ingredient.

 The

Attempt to ascertain whether sulphur form water by burning in dry oxigen.

The oily substances in combustion produce two or three times their weight of carbonic acid and some water. I endeavoured to ascertain, whether water was formed in the combustion of sulphur in oxigen gas, dried by exposure to potash; but in this case sulphureous acid is produced in much larger quantities than sulphuric acid, and this last product is condensed with great difficulty. In cases, however, in which I have obtained, by applying artificial cold, a deposition of acid in the form of a film of dew in glass retorts out of the contact of the atmosphere, in which sulphur had been burned in oxigen gas hygrometrically dry, it has appeared to me less tenacious and lighter than the common sulphuric acid of commerce, which in the most concentrated form in which I have seen it, namely, at 1·855, gave abundance of hidrogen as well as sulphur at the negative surface in the voltaic circuit, and hence evidently contained water.

Reddening of the litmus might be by sulphuretted hidrogen.

The reddening of the litmus paper, by sulphur that had been acted on by voltaic electricity, might be ascribed to its containing some of the sulphuretted hidrogen formed in the process; but even the production of this gas, as will be immediately seen, is an evidence of the existence of oxigen in sulphur.

Potassium heated in sulphuretted hidrogen.

In my early experiments on potassium, procured by electricity, I heated small globules of potassium in large quantities of sulphuretted hidrogen, and I found that sulphuret of potash was formed; but this might be owing to the water dissolved in the gas, and I ventured to draw no conclusion till I had tried the experiment in an unobjectionable manner.

Perfectly dried,

I heated four grains of potassium in a retort of the capacity of twenty cubical inches; it had been filled after the usual processes of exhaustion with sulphuretted hidrogen, dried by means of muriate of lime that had been heated to whiteness; as soon as the potassium fused, white fumes were copiously emitted, and the potassium soon took fire, and burnt with a most brilliant flame, yellow in the centre and red towards the circumference*.

took fire,

The

* In the Moniteur, May 27, 1808, in the account of M. M. Gay-Lussac and Thenard's experiments, it is mentioned, that potassium

The diminution of the volume of the elastic matter, in this operation, did not equal more than two cubical inches and a half. A very small quantity of the residual gas only was absorbable by water. The nonabsorbable gas was hidrogen, holding a minute quantity of sulphur in solution.

leaving hidrogen gas,

A yellow sublimate lined the upper part of the retort, which proved to be sulphur. The solid matter formed was red at the surface like sulphuret of potash, but in the interior it was dark gray, like sulphuret of potassium. The piece of the retort containing it was introduced into a jar inverted over mercury, and acted upon by a small quantity of dense muriatic acid, diluted with an equal weight of water, when there were disengaged two cubical inches and a quarter of gas, which proved to be sulphuretted hidrogen.

and sulphur sublimed. Solid matter.

In another experiment, in which eight grains of potassium were heated in a retort of the capacity of twenty cubical inches, containing about nineteen cubical inches of sulphuretted hidrogen, and a cubical inch of phosphuretted hidrogen, which was introduced for the purpose of absorbing the oxigen of the small quantity of common air admitted by the stop-cock, the inflammation took place as before; there was a similar precipitation of sulphur on the sides of the retort; the mass formed in the place of the potassium was orange externally, and of a dark gray colour internally, as in the last instance; and when acted on by a little water holding muriatic acid in solution, there were evolved from it five cubical inches only of sulphuretted hidrogen.

Experiment repeated.

Both these experiments concur in proving the existence of a principle in sulphuretted hidrogen, capable of destroying partially the inflammability of potassium, and of producing upon it all the effects of oxigen; for had the potassium combined merely with pure combustible matter, it ought, as will be seen distinctly from what follows, to have evolved by the action of the acid a volume of sulphuretted

Principle in sulphuretted hidrogen producing the effects of oxigen.

potassium absorbs the sulphur and a part of the hidrogen of sulphuretted hidrogen; but the phenomenon of inflammation is not mentioned, nor are the results described.

hidrogen

hidrogen, at least equal to that of the hidrogen, which an equal weight of uncombined potassium would have produced by its operation upon water.

Sulphur heated n hidrogen.

Sulphuretted hidrogen, as has been long known to chemists, may be formed by heating sulphur strongly in hidrogen gas. I heated four grains of sulphur in a glass retort, containing about twenty cubical inches of hidrogen, by means of a spirit lamp, and pushed the heat nearly to redness. There was no perceptible change of volume in the gas after the process; the sulphur that had sublimed was unaltered in its properties, and about three cubical inches of an elastic fluid absorbable by water were formed: the solution reddened litmus, and had all the properties of a solution of pure sulphuretted hidrogen. Now if we suppose sulphuretted hidrogen to be constituted by sulphur dissolved in its unaltered state in hidrogen, and allow the existence of oxigen in this gas; its existence must likewise be allowed in sulphur, for we have no right to assume, that sulphur in sulphuretted hidrogen is combined with more oxigen than in its common form: it is well known, that, when electrical sparks are passed through sulphuretted hidrogen, a considerable portion of sulphur is separated, without any alteration in the volume of the gas. This experiment I have made more than once, and I found that the sulphur obtained, in fusibility, combustibility, and other sensible properties, did not perceptibly differ from common sublimed sulphur.

Oxigen in sulphur

accounts for its intense ignition with potassium.

According to these ideas, the intense ignition produced by the action of sulphur, on potassium and sodium, must not be ascribed merely to the affinity of the metal of the alkalis for its basis, but may be attributed likewise to the agency of the oxigen that it contains.

The minute examination of the circumstances of the action of potassium and sulphur likewise confirms these opinions.

Farther confirmed.

When two grains of potassium and one of sulphur were heated gently in a green glass tube filled with hidrogen, and connected with a pneumatic apparatus, there was a most intense ignition produced by the action of the two bodies, and one eighth of a cubical inch of gas was disengaged

engaged, which was sulphuretted hidrogen. The compound was exposed in a mercurial apparatus to the action of liquid muriatic acid; when a cubical inch and a quarter of aeriform matter was produced, which proved to be pure sulphuretted hidrogen.

The same experiment was repeated, except that four grains of sulphur were employed instead of one. In this case, a quarter of a cubical inch of gas was disengaged during the process of combination; and when the compound was acted upon by muriatic acid, only three quarters of a cubical inch of sulphuretted hidrogen were obtained.

Now, *sulphuret* of potash produces sulphuretted hidrogen by the action of an acid; and if the sulphur had not contained oxigen, the hidrogen evolved by the action of the potassium in both these experiments ought to have equalled at least two cubical inches, and the whole quantity of sulphuretted hidrogen ought to have been more: and that so much less sulphuretted hidrogen was evolved in the second experiment, can only be ascribed to the larger quantity of oxigen furnished to the potassium by the larger quantity of the sulphur.

Several experiments made with similar results.

I have made several experiments of this kind with similar results. Whenever equal quantities of potassium were combined with unequal quantities of sulphur, and exposed afterward to the action of muriatic acid, the largest quantity of sulphuretted hidrogen was furnished by the product containing the smallest proportion of sulphur; and in no case was the quantity of gas equal in volume to the quantity of hidrogen, which would have been produced by the mere action of potassium upon water.

Composition of sulphur.

From the general tenour of these various facts, it will not be, I trust, unreasonable to assume, that sulphur, in its common state, is a compound of small quantities of oxigen and hidrogen with a large quantity of a basis, that produces the acids of sulphur in combustion, and which, on account of its strong attractions for other bodies, it will probably be very difficult to obtain in its pure form.

In metallic combinations even, it still probably retains its oxigen and part of its hidrogen. Metallic sulphurets can only be partially decomposed by heat, and the small quantity

quantity of sulphur evolved from them in this case when perfectly dry and out of the contact of air, as I found in an experiment on the sulphuret of copper and iron, exists in its common state, and acts upon potassium, and is affected by electricity, in the same manner as native sulphur.

4. *Analytical Experiments on Phosphorus.*

Phospohrus analogous to sulphur.

The same analogies apply to phosphorus as to sulphur, and I have made a similar series of experiments on this inflammable substance.

Acted on by the pile,

Common electrical sparks, passed through phosphorus, did not evolve from it any permanent gas; but when it was acted upon by the voltaic electricity of the battery of five hundred plates in the same manner as sulphur, gas was produced in considerable quantities, and the phosphorus became of a deep red brown colour, like phosphorus that has been inflamed and extinguished under water. The gas examined proved to be phosphuretted hidrogen, and in one experiment, continued for some hours, a quantity estimated to be nearly equal to four times the volume of the phosphorus employed was given off. The light of the voltaic spark in the phosphorus was at first a brilliant yellow, but as the colour of the phosphorus changed, it appeared orange.

evolved phosphuretted hidrogen.

Potassium heated in phosphuretted hidrogen.

I heated three grains of potassium in sixteen cubical inches of phosphuretted hidrogen; as soon as it was fused, the retort became filled with white fumes, and a reddish substance precipitated upon the sides and upper part of it. The heat was applied for some minutes. No inflammation took place*. When the retort was cool, the absorption was found to be less than a cubical inch. The potassium externally was of a deep brown colour, internally it was of a dull lead colour. The residual gas had lost its property of spontaneous inflammation, but seemed still to contain a small quantity of phosphorus in solution.

* It is stated, in the account before referred to of M. M. Gay-Lussac and Thenard's experiments, that potassium inflames in phosphuretted hidrogen. My experiments upon this gas have been often repeated. I have never perceived any luminous appearance; but I have always operated in daylight.

The phosphuret acted upon over mercury by solution of muriatic acid evolved only one cubical inch and three quarters of phosphuretted hidrogen.

Phosphuretted hidrogen contains oxigen.

From this experiment there is great reason to suppose, that phosphuretted hidrogen contains a minute proportion of oxigen, and consequently that phosphorus likewise may contain it; but the action of potassium on phosphorus itself furnishes perhaps more direct evidences of the circumstance.

Phosphorus fused with potassium.

One grain of potassium and one grain of phosphorus were fused together in a proper apparatus. They combined with the production of the most vivid light and intense ignition. During the process one tenth of a cubical inch of phosphuretted hidrogen was evolved. The phosphuret formed, exposed to the action of diluted muriatic acid over mercury, produced exactly three tenths of a cubical inch of phosphuretted hidrogen.

Experiment repeated.

In a second experiment, one grain of potassium was fused with three grains of phosphorus; in this case nearly a quarter of a cubical inch of phosphuretted hidrogen was generated during the ignition. But from the compound exposed to muriatic acid, only one tenth of a cubical inch could be procured.

Phosphorus contains oxigen.

Now it is not easy to refer the deficiency of phosphuretted hidrogen in the second case to any other cause, than to the supply of oxigen to the potassium from the phosphorus: and the quantity of phosphuretted hidrogen evolved in the first case is much less than could be expected, if both potassium and phosphorus consisted merely of pure combustible matter.

Phosphoric acid may contain water.

The phosphoric acid, formed by the combustion of phosphorus, though a crystalline solid, may still contain water. The hidrogen evolved from phosphorus by electricity proves indeed, that this must be the case; and though the quantity of hidrogen and oxigen in phosphorus may be exceedingly small, yet they may be sufficient to give it peculiar characters; and till the basis is obtained free, we shall have no knowledge of the properties of the pure phosphoric element.

5. *On*

5. *On the States of the carbonaceous Principle in Plumbago, Charcoal, and the Diamond.*

Plumbago, charcoal, and diamond,

The accurate researches of Messrs. Allen and Pepys have distinctly proved, that plumbago, charcoal, and the diamond produce very nearly the same quantities of carbonic acid, and absorb very nearly the same quantities of oxigen in combustion.

consist principally of the same element,

Hence it is evident, that they must consist principally of the same kind of elementary matter; but minute researches upon their chemical relations, when examined by new analytical methods, will, I am inclined to believe, show, that the great difference in their physical properties does not merely depend upon the differences of the mechanical arrangement of their parts, but likewise upon differences in their intimate chemical nature.

but with chemical differencies.

Plumbago acted upon by the pile in vacuo.

I endeavoured to discover, whether any elastic matter could be obtained from plumbago very intensely ignited by the Voltaic battery in a Torricellian vacuum: but though the highest power of the battery of five hundred was employed, and though the heat was such, as in another experiment instantly melted platina wire of $\frac{1}{60}$th of an inch in diameter, yet no appearance of change took place upon the plumbago. Its characters remained wholly unaltered, and no permanent elastic fluid was formed.

Heated with potassium in hidrogen gas.

I heated one grain of plumbago, with twice its weight of potassium, in a plate glass tube connected with a proper apparatus, and I heated an equal quantity of potassium alone in a tube of the same kind, for an equal length of time, namely, eight minutes. Both tubes were filled with hidrogen: no gas was evolved in either case. There was no ignition in the tube containing the plumbago, but it seemed gradually to combine with the potassium. The two results were exposed to the action of water; the result from the plumbago acted upon that fluid with as much energy as the other result, and the two volumes of elastic fluids were 1·8 cubical inch and 1·9 cubical inch; and both gave the same diminution by detonation with oxigen, as pure hidrogen. Two grains of potassium, by acting upon water, would have produced two cubical inches and one eighth

of

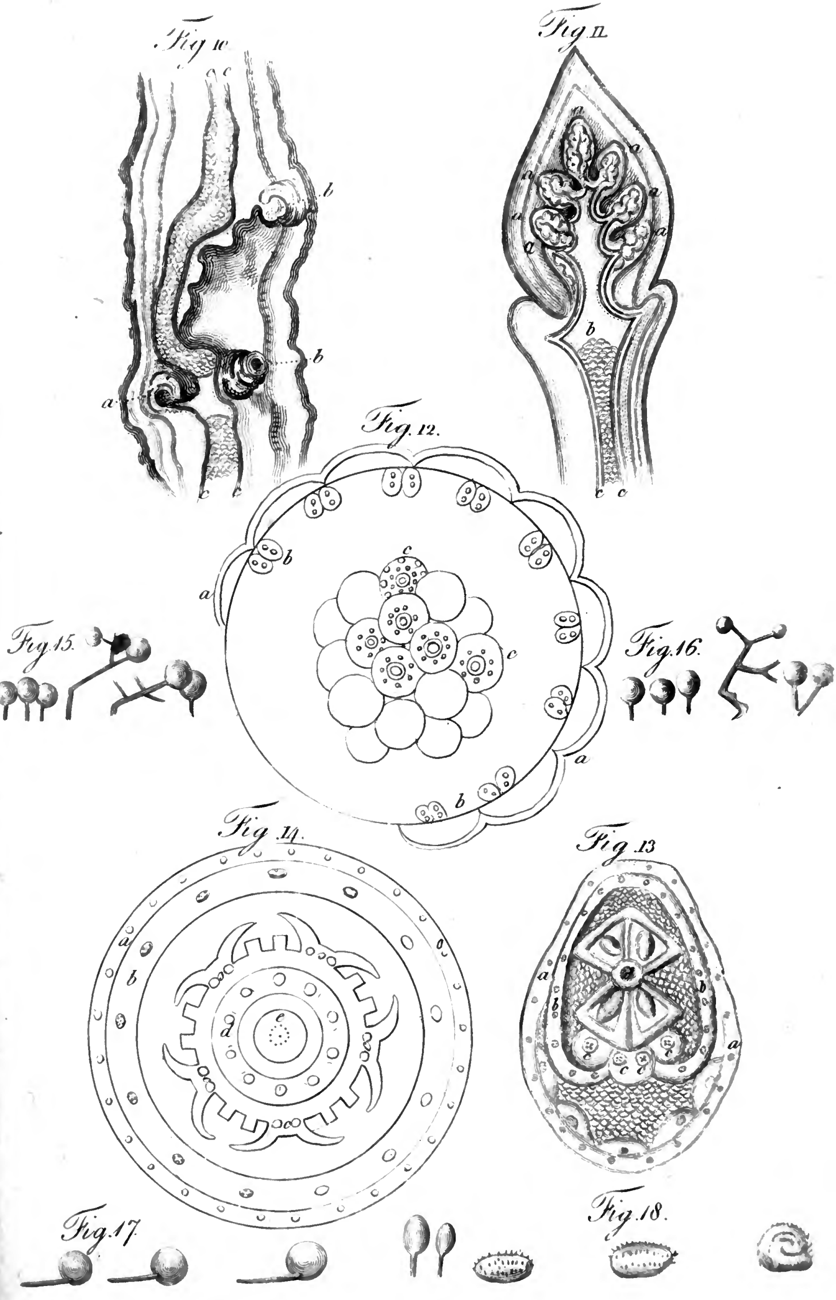


Fig. 10.
Fig. 11.
Fig. 12.
Fig. 13.
Fig. 14.
Fig. 15.
Fig. 16.
Fig. 17.
Fig. 18.

of hidrogen gas; the deficiency in the result, in which potassium alone was used, must be ascribed to the loss of a small quantity of metal, which must have been carried off in solution in the hidrogen, and perhaps, likewise, to the action of the minute quantity of metallic oxides in the plate glass. The difference in the quantity of hidrogen given off in the two results is however too slight, to ascribe it to the existence of oxigen in the plumbago.

I repeated this experiment several times with like results, and in two or three instances examined the compound formed. It was infusible at a red heat, had the lustre of plumbago. It inflamed spontaneously, when exposed to air, generated potash, and left a black powdery residuum. It effervesced most violently in water, and produced a gas, which burnt like pure hidrogen. The experiment repeated.

When small pieces of charcoal from the willow, that had been intensely ignited, were acted upon by Voltaic electricity in a Torricellian vacuum, every precaution being taken to exclude moisture from the mercury and the charcoal, the results were very different from those occurring in the case of plumbago. Charcoal acted upon by the pile in vacuo.

When plumbago was used, after the first spark, which generally passed through a distance of about one eighth of an inch, there was no continuation of light, without a contact or an approach to the same distance; but from the charcoal a flame seemed to issue of a most brilliant purple, and formed, as it were, a conducting chain of light of nearly an inch in length, at the same time that elastic matter was rapidly formed, some of which was permanent. After many unsuccessful trials, I at length succeeded in collecting the quantity of elastic fluid given out by half a grain of charcoal; the process had been continued nearly half an hour. The quantity of gas amounted to nearly an eighth of a cubical inch; it was inflammable by the electric spark with oxigen gas, and four measures of it absorbed three measures of oxigen, and produced one measure and a half of carbonic acid. The charcoal in this experiment had become harder at the point, and its lustre, where it had been heated to whiteness, approached to that of plumbago. A purple flame formed, and elastic matter evolved.

I heated two grains of potassium together with two grains of Charcoal heat-

ed with potassium.

of charcoal, for five minutes; and to estimate the effects of the metallic oxides and potash in the green glass tube, I made a comparative experiment, as in the case of plumbago; but there was no proof of any oxigen being furnished to the potassium from the charcoal in the process, for the compound acted upon water with great energy, and produced a quantity of inflammable gas, only inferior by one twelfth to that produced by the potassium, which had not been combined with charcoal, and which gave the same diminution by detonation with oxigen; and the slight difference may be well ascribed to the influence of foreign matters in the charcoal. There was no ignition in the process, and no gas was evolved.

Compound produced.

The compound produced in other experiments of this kind was examined. It is a conductor of electricity, is of a dense black, inflames spontaneously, and burns with a deep red light in the atmosphere*.

Diamond could not be acted on by the pile.

The nonconducting nature of the diamond, and its infusibility, rendered it impossible to act upon it by voltaic electricity; and the only new agents which seemed to offer any means of decomposing it, were the metals of the alkalis.

Heated with potassium,

When a diamond is heated in a green glass tube with potassium, there is no elastic fluid given out, and no intensity of action; but the diamond soon blackens, and scales seem to detach themselves from it, and these scales, when examined in the magnifier, are gray externally, and of the colour of plumbago internally, as if they consisted of plumbago covered by the gray oxide of potassium.

in hidrogen gas.

In heating together three grains of diamonds in powder, and two grains of potassium, for an hour, in a small retort of plate glass filled with hidrogen, and making the comparative trial with two grains of potassium heated in a similar apparatus, without any diamonds, I found, that the potassium which had been heated with the diamonds produced, by its action upon water, one cubical inch and $\frac{3}{10}$ of in-

* In the Bakerian Lecture for 1807, I have mentioned the decomposition of carbonic acid by potassium, which takes place with inflammation. If the potassium is in excess in this experiment, the same pyrophorus as that described above is formed.

flammable

flammable air, and that which had been exposed to heat alone, all other circumstances being similar, evolved nearly one cubical inch and $\frac{7}{10}$, both of which were pure hidrogen.

A similar experiment.

In another experiment of a similar kind, in which fragments of diamonds were used in the quantity of four grains, the potassium became extremely black from its action upon them during an exposure to heat for three hours, and the diamonds were covered with a grayish crust, and when acted upon by water and dried, were found to have lost about $\frac{28}{100}$ of a grain in weight. The matter separated by washing, and examined, appeared as a fine powder of a dense black colour. When a surface of platina wire was covered with it, and made to touch another wire in the Voltaic circuit, a brilliant spark with combustion occurred. It burnt, when heated to redness in a green glass tube filled with oxigen gas, and produced carbonic acid by its combustion.

Plumbago.

These general results seem to show, that in plumbago the carbonaceous element exists merely in combination with iron, and in a form which may be regarded as approaching to that of a metal in its nature, being conducting in a high degree, opaque, and possessing considerable lustre.

Charcoal.

Charcoal appears to contain a minute quantity of hidrogen in combination. Possibly likewise, the alkalis and earths produced during its combustion exist in it not fully combined with oxigen; and according to these ideas, it is a very compounded substance, though in the main it consists of the pure carbonaceous element.

Diamond.

The experiments on the diamond render it extremely likely, that it contains oxigen; but the quantity must be exceedingly minute, though probably sufficient to render the compound nonconducting: and if the carbonaceous element in charcoal and the diamond be considered as united to still less foreign matter in quantity, than in plumbago, which contains about $\frac{1}{20}$ of iron, the results of their combustion, as examined independently of hygrometrical tests, will not differ perceptibly.

Minute differences in composition may greatly alter

Whoever considers the difference between iron and steel, in which there does not exist more than $\frac{1}{200}$ of plumbago, or the difference between the amalgam of ammonium and mercury,

external appearance.

mercury, in which the quantity of new matter is not more than $\frac{1}{1000}$, or that between the metals and their suboxides, some of which contain less than $\frac{1}{10}$ of oxigen, will not be disposed to question the principle, that minute differences in chemical composition may produce great differences in external and physical characters.

(To be continued in our next.)

II.

On the Stem of Trees; with an Attempt to discover the Cause of Motion in Plants. By MRS. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

Method of dividing the stem of trees.

THE manner in which Linnæus divided the stem of trees was naturally suggested by its appearance to the eye, *little aided* by glasses: cortex, the rind; liber, the bark; lignum, the wood; and medulla, the pith. But at this time, that our magnifiers are so perfected; nature points out a more regular division, and one marked not only by the form, but by the difference of the *juices*, with which the parts are swelled. Indeed so different are the purposes to be effected, and so clear are the divisions nature has made; that, when seen much magnified, *they appear to me* directly to strike the mind, and convince the reason; provided the study is pursued in a manner, that will enable the person, by a view of the different parts properly prepared, to judge sanely on the subject. The vegetable cuttings sold with the solar microscope will do very well for superficial learners, but no person can understand the nature of plants, or expect to profit from knowledge so obtained, who does not cut his own specimens, and generally from fresh plants. It is laborious and troublesome, and requires great care; but I have never a moment repented the time so expended, as from dried cuttings much of the real nature *and all the motion* escape. Still both are to be consulted; and the proper method is perhaps to compare them together. I copy from no book, every experiment has been made by myself, and carefully repeated a number of times: I may perhaps

To judge from both dried and fresh cuttings.

perhaps be accused of presumption, in *venturing* to *introduce* so *many new ideas;* and depending thus *on myself* only; but I recount merely what I have seen in a very *good solar microscope*; if my deductions are false, I detail my reasons; and every reader may judge for himself. It is to the *great magnifying powers* I am indebted; and every one (with the same instrument) may prove the truth of what I advance.

Division of the stem.

I shall divide the stem of trees into 6 parts; 1st the rind; 2nd the bark and inner bark; 3d the wood; 4th the spiral nerves; 5th the *nerves* or *circle of life;* 6th the pith. The rind is I conceive merely an outward covering to the tree, to preserve its moisture, that the sun may not evaporate its juices. It is true, that the same is continued under ground; but it may be as useful there to prevent the entrance of the dust and earth, and pressure of stones, or the injury of insects. It is composed of rows of cylinders with a single line to divide them, and they are filled with a clear and pellucid liquor. There are seldom more than four or five layers of vessels; but it is in general so covered with parasite plants, and powdered lichens, that its thickness is often more than doubled; and it is not fit for examination, till divested of all extraneous matter. It is the rind thickened that forms much of the armature of plants. It appears by no means necessary to plants, as there are a number in which the bark serves as a covering instead of a rind; it is not therefore *essential* to them. Though to trees it must be so reckoned.

Division of the rind.

Divisions of the bark.

2d The bark and inner bark, though certainly very different as to *form*, are the same in juice; and being so nearly allied, I shall treat them as one. They are truly of the first consequence in the tree. They are the origin of the leaves; the lengthened vessels of the bark and inner bark, forming the *interlacing* vessels of the leaf, while the juice concentrated and thickened produces the pabulum of the leaf, as I endeavoured to show in my last paper. The juice of the bark is I conceive the blood of the tree. It is here alone are produced the gums, the resins, the oil, the milk, in short all that truly belongs to the tree; gives taste to it; all I conceive that makes one plant different from another;

another; and its virtues, if I may so express myself. The bark is *generally* green, the inner bark white, yellow, or green. The former consists of vessels crossing each other; the latter of bundles of vessels of two sizes, the large ones being formed in a very peculiar manner. They consist of broad cylinders, having a bottom with a hole in it, through which the liquid passes, though not with perfect ease. On exposing several pieces of the inner bark to the solar microscope, the moment I turned the light on the specimen, the juice of which had before proceeded up the pipes rather slowly, it was suddenly propelled forward with a force truly astonishing. When I increased the heat and light by pointing the full focus of the rays on the vessels, the power of the heat was too strong, and broke through the side divisions, inundating the specimen: but when I merely kept up a proper degree of light and heat, it was curious to observe the liquid pass from pipe to pipe, in one regular and easy flow; making a little stop as it issued through the straitened apertures at the bottom of the vessels. I have often stood more than an hour watching the current, (which passes however much slower than the sap does) nor could I perceive, that it required (while the heat and light were on it) any additional expedient to hasten it; but in the night, when both are wanting, the pressure Mr. Knight mentions from the bastard grain is (I should suppose) very likely to assist or quicken its flow; and as at night it is pressed against the cylinders, it is at this time (I should conceive) it would have its effect. This part is however formed in the wood only; but the contraction at the bottom of the large vessels of the inner bark, it is probable may serve the same purpose, that of forcing the liquid forward, by lessening the apertures, and giving therefore more impetus to the current. The vessels are also of great thickness in proportion to their size; and have on them a peculiar circular thing resembling a cullender full of very diminutive holes, so small that no liquid could pass them; but in viewing the thick juice, that runs through these pipes, I observed many bubbles of air, which, as the heat increased or diminished *their size*, *accelerated* or *retarded* the flow of the liquid. Might not these apertures be designed for the entrance of air

Extraordinary flow of the liquid.

Curious formation of the vessels in the bark.

Bubbles of air in the liquid.

air to promote this purpose? The thickness of these vessels is such, as almost to conceal the darkness of the liquid that runs through them. To see their forms well, it is sometimes necessary to clear out their contents, which is best done by placing the specimens in a basket fastened down in a running stream, or boiling them thoroughly, and then throwing them into green wax perfectly melted. When this succeeds, it makes excellent specimens for the cabinet.

Though half fearful to give an opinion absolutely contradictory to one whose abilities I so *much respect as Mirbel's*, yet I must think he is mistaken, when he says: Il y a des plantes qui ont les mêmes sucs dans toutes leur parties." I never could find this; and though the potent smell of the liquid belonging to the bark will often extend to other parts of the plant, it generally vanishes if kept separate for a day, or grows so faint in comparison with the real liquid, as to prove it is not an ingredient. Nor can I understand why he should suppose, that the tubes or cylinders of the inner bark are merely vacancies of the ordinary vessels; for they are exactly the same, and occupy the same place; their peculiar shape and office attend them every where; and there are no vessels like them in any other part of the tree or shrub. I have mentioned only the vessels of the inner bark, because their form is unusual; but the vessels of the bark are more simple and smaller, and divided by a line or two, running longitudinally between them. How the gums, resins, oils, milk, &c. are formed, I am not chymist sufficient to give any clear idea concerning; but the labours of Dr. Thomson seem more to elucidate this subject, than those of any other author I am acquainted with. Nothing can be more admirable than the manner in which he accounts for sugar in plants; it is exemplified each day in those that are out of health. Mirbel has also a very valuable paper on the subject.

Liquids peculiar to each part.

3d I now turn to the wood of the stem. This is marked by nature with such strong lines, it is hardly possible to mistake its parts. Place the stem of any plant in a coloured liquor, and every vessel which conveys the sap from the earth to the top of the tree will be marked and tinged. The

Formation and use of the wood vessels.

The sap is the nourishment those vessels convey; it is a thin waterish liquor, which is probably the juices of the earth, *medicated* into this form, as most suitable to the life it is to support. I suppose it is different in each different soil; but though I have often tried "by separating the wood from the rest of the stem, and then macerating it, to draw forth the liquor from the same tree in different soils," I never could perceive there was the change one should naturally expect.

Two different stripes in the wood.

On dissecting the wood; two different kinds of stripes present themselves, some circular, an additional one being each year added, which timber merchants call the silver grain; and another from the circumference to the centre, at least from the first line of the wood to the pith, which they call the bastard grain. The first is the yearly stripe, and I had an opportunity in a large wood that was felled of observing the truth, not only of one stripe being added each year, but that the stripe was large or small, according to the exposure of the tree, and the favourableness of the season. The wood had been planted at two different times, one part 88 years, and the other 56; and each tree was exactly marked according to its age, except *three or four which* gave not the number of stripes specified, and were afterward proved to have been planted instead of others, that had been broken and cut down. In exposed situations the *west side* was much *narrower* in several of the trees; and in the forwarder trees the N. and N. E. was the most *crowded*, I mean, that in measuring the diameter of the wood, it was less on one side of the circumference, than on the other. In several trees there was sometimes only a half circle; and in three different oaks, a rotten part having caused the line of life to leave its situation, part of the pith had followed it, and it had formed two piths, with many rows of wood between. The bastard stripe consists I think of two lines, or strings, with a little scale between them; and they appear from their extreme susceptibility to be formed of the same leatherlike substance as the spiral vessels. Mr. Knight is of opinion, that they are scales only, and he is too exact an observer to be contradicted lightly; but as he mentions their pressing close (which they certainly do) to the

Leatherlike strings of the bastard grain.

the cylinders at night and in cold weather, they would equally have the effect required; *that* of *supplying by their pressure the want of the sun's rays.* The wood vessels are far more simply made than those of the bark; they are very narrow cylinders; and the last two rows next to the circle of life are sap vessels covered by the spiral ones. The horse chesnut has three or four rows, and they appear to be in quantity according to the size of the leaves. It is indeed difficult to ascertain them exactly even in the solar microscope, as it is in *unwinding* them alone they *can be known*; and their extreme fineness confuses. This has however caused the spiral vessels to be taken for sap vessels. It was a great pleasure to me to find, that neither Mr. Knight nor Mirbel was of this opinion. I believe there can be no doubt, that they are *solid strings*, and hold *no liquid*. When wood is very aged, it grows so compact, that it is difficult without preparation to see the open mouths of the vessels. The wood should then be cut in thin slices, and laid in a very dry place; and it is wonderful how this will stretch the upper end of the cylinders; but fresh cut specimens, *if examined directly*, will almost always be sufficiently visible. *If much magnified*, and cut longitudinally, it is truly wonderful to see the effect of light and heat on the wood vessels; how immediately on turning the light on the glass, the flow of sap is accelerated, and with what perfect ease it runs up vessels so diminutive, that to measure them is almost impossible. Is it not most wonderful to consider the force necessary to carry up this sap, when the vessels are formed of a substance *so thin*, *so transparent*, that it would appear impossible to confine a liquid within it; and yet that, without being worn out by *friction*, it will bear this force exerted against it, for eighty years together, without showing any signs of decay, a term which many trees will sustain? This indeed proclaims its author, and should make the atheist fall down and worship. A few of the wood vessels are separated, and run with the spiral vessels as nourishing vessels to each leaf, as I have shown in my last; but this diverts but little of the sap from its chief current, which flows on; its last purpose being to form the stamen, and the curious powder that apertains

Spiral vessels not sap vessels.

All cut with an instrument not a knife.

to

to it; and afterward to lend its principal aid to the formation of the fruit and seed. For it is this last, that is the grand and finishing work of nature, to which all the rest tends but as a means to the great accomplishment of producing new vegetable lives.

The spiral vessels.

The spiral vessels are a quantity of solid strings coiled up into a spiral form. I cannot but suppose them of a leatherlike substance, and to be found rolled round the last few rows of sap vessels. In this manner they run up the stems of trees and plants of every kind (with a few exceptions) and thence into every leaf and flower. They are singly too small for the naked eye; they run into every fibre of the leaf, and are fastened at the edges, by which means, crossing like a spider's web in every direction through the vessels, they can draw the leaves in any way that is necessary to them. In the larger vessels they are in sets of ten or twelve, but in the smaller only three or four to each vessel. In the cabbage leaf and in the burdock they are in bundles almost as thick as a packthread; but in smaller leaves they are properly proportioned. The more sensitive the leaf, the more they are *coiled up.* These are (I truly believe) the cause.

The cause of motion in leaves and flowers.

The spiral vessels are *(I truly believe) the cause of motion in plants.* I do not mean to say, that there is no motion in plants but what arises from them; but I am fully persuaded, that the *greatest part of the motion in leaves and flowers* proceeds from the management of this spiral wire. I shall now detail my reasons for this persuasion.

Spiral vessels found in no leaves that do not turn.

1*st.* The spiral vessels are not to be found in any plants, to which motion is unnecessary. They are never found in any of the firs, in any of the water plants that spread their leaves on the top of the water, in any of the sea weeds, or in any of the lichens; I think too they are not found in the scolopendrums, or in the lemnas; though at first I took the line of life, that runs into the leaf to form the flower, for one. The grasses also, having no cause for turning their leaves, are wholly without them.

2*d.* If a plant in a window, having all its leaves with their backs turned from the light, is moved, and placed so as to turn them to the sun, they will in a few hours regain their

their former position; reverse it, and it will now want double the time to bring them right; change the order a third time, and though the plant will not in any manner have suffered, yet the leaves will be long regaining their pristine force. Few can move after the third or fourth regression; and why? because the *spiral, like elastic vessels,* were so relaxed by the operation, as to have lost all power of coiling into their usual form.

3*d*. I have observed that those leaves, that have the most motion, have also the most of these spiral vessels, and have them most twisted. This is particularly seen in the *populus tremula*; the leaf stalk, though small, is full of them, and so hard twisted, that I have known the stalk to measure a *quarter of an inch* difference in length between the middle of the *day* and a cold *evening*. This could arise only from the untwisting of the spiral wire; and few plants have more motion, indeed it has far more than can fairly be attributed to its long leaf stalk.

Most motion in spiral vessels most twisted.

4*th*. I took a vine leaf, and without separating it from its parent plant, I merely divided the spiral vessels, *without touching the nourishing ones*; it never from that moment either *turned*, or *contracted*; and when placed with its back to the light, it *remained* in this position, though it was long before it decayed. Both electricity and galvanism draw up these leaves, as if they were leather: but it is the spiral fibres, not the *cuticles*; for after I took from a leaf all the spiral wire, the leaf did *not* contract at all. Bonnet was convinced, that all the motion of plants might be given by the means of threads, but microscopes were not so perfect then as to give him the delight of knowing, that he had guessed the operations of nature. He made an artificial leaf and flower, that would move by the contrivance of threads that passed through all the larger vessels, and by this means they effected every movement common to either. But his were *plain threads*, not a spiral wire, the elastic power of which is well known to every person: nor had he an idea, that such vessels existed, but thought it was the contraction and elongation of the upper and under cuticle of the leaf; but this is certainly not the case, as I have proved above, that it has *no* such powers. There are insects

Leaves neither turn nor move if the spiral wires be cut.

Bonnet's contrivance.

Insects contract the spiral wire.

sects in the currant, and many other leaves, that show the power of the elastic wire, *as much as any thing yet mentioned.* Nature has taught them, to draw up these spiral vessels, to make themselves nests, in which to deposit their eggs and young; and any one may see in what manner it is done, and how the leaf is shortened.

Heat contracts the spiral wire.

5th. I took a quantity of these spiral vessels from a cabbage leaf, and placed them on a long netting needle in my solar microscope, that the motion might be extremely visible, and made my assistant hold a candle to the other end of the needle. As the heat approached it, the vessels were agitated inexpressibly, and appeared wreathing like a worm, till with one effort they flung themselves off the needle. The fresh water conferva, and the dodder tribe, are the only plants without leaves, that have the *spiral vessels*, that I am acquainted with. The former is almost formed of it; and the sensitive plants have scarce more motion than the common green conferva. I have seen it draw itself up, then turn with a sudden motion, and surround a pin, coiling up it like a worm; and it will continue to move thus for more than an hour after it is taken from the water. In the leaf stem of the geranium cordifolium the spiral vessels are so very tough, and so very tightly coiled, that I have by great care drawn up the leaf by their means; but this is difficult to be done. Some may imagine, that these spiral wires are too delicate to turn the leaf or flower; but can any one say this, who is in the constant habit of dissecting plants? or who has seen the extreme delicacy of flowers, and yet the force they will exert, or the tenderness of the young shooting plant, and yet the strength with which it will force its way through brick and mortar, and even through solid stones? The works of man are effected by using strong materials, when *powerful ends* are in view; but the works of God are performed in a more wonderful manner, the most delicate means produce the greatest ends. Look on the vegetable cuttings; it is the aggregate of such pieces which forms our ships, and which stands the united attacks of winds and waves. View the metals, as they first grow or shoot into crystallization in the Arbor Dianæ or the leaden tree; who would recognize the destructive bomb, or the hardened

Strength of many apparently weak parts of vegetables.

hardened coin? But the mind that is accustomed to see them in their first delicate forms produce great effects, will not doubt what the Almighty power may fit them for.

In detailing the arguments that tend to prove, that the spiral wire is the cause of motion in plants, I must suggest one, which will at least clear it from all *improbability*. To those to whom the energy, strength, delicacy, and susceptibility of Captain Kater's hygrometer is known, it will offer a certain proof of the possibility of such an *existing power;* since that little instrument is acted upon by the power moisture has of untwisting the awn of a grass brought from India. Now if the most trifling change of moisture can untwist one sort of vegetable fibre, and by this means manage an instrument, why should not a quantity of similarly formed fibres or spiral wires produce the same effect on leaves and flowers? Captain Kater's hygrometer moves very sensibiy if a finger is placed within *half an inch of the fibre:* now the most sensitive plant we have will not move but with the *touch:* though I doubt not in its natural soil and climate it is more sensible: but in the sensitive plants there is a peculiarity in the joint, which helps to produce that regularity of movement which is the most curious circumstance in its formation; this I hope to explain in my next. My only doubt is, I confess, whether the power that governs the spiral wire is light, heat, or moisture? I am rather inclined to think it is moisture; though of course light and heat must have very great influence, as no change of either can happen, without its increasing or diminishing the moisture of the atmosphere.

Argument from Kater's hygrometer, that the spiral wire is the motive organ.

I fear I have tired the reader; but I have not produced half the proofs I might bring forward to show, "that if the spiral vessels are *the origin of motion in both leaves and flowers,*" flowers may be made to change their position with every variation of light and heat, even *more* than *leaves;* and in the acacia I have made the leaves and flowers droop in the middle of the day, by holding a wet napkin suspended over them after I had completely shaded them; and by carrying flowers into an ice house, they will distinctly prove what part is affected.

Flowers.

The

Circle of life.

The next part is the small circle of vessels situate between the wood and the pith, or rather between the spiral vessels and the pith; which plays so very conspicuous a part in the history of the beginning seed, as I hope to have proved in my first letter; and which I have ventured to call the circle of life. I gave before the strongest proof I could, that a plant cannot exist a day without it; and that, if taken away at a very early age, it will *not* (like every other part) grow again: but when older it will certainly renew itself. It is very curious, that every botanical anatomist has drawn these lines without giving them a name, or otherwise noticing them; they attributed all their powers to the pith, which, from the scanty term of its existence, and its being perpetually impeded in its progress, to make way for the flower bud, can evidently have little power. But it was probably their extreme delicacy that caused them to be overlooked *by all but Hill*, whose admirable treatise on the woods it is quite wonderful should be disregarded. The circle of life consists of rows of little cylinders, that have their own peculiar juice, generally of an austere quality. From this part all branches take their rise, and all wood threads grow. They run up (see Pl. IX, Fig. 10 and 11) into all flower buds, but never approach the leaf bud. When they enter the former, they make their way distinctly to each separate flower, forming the pistil, and after depositing in each seed the *line* which is the first origin of life, they are afterward impregnated, or gain the power of giving life, by the juice of the stamen, which runs through the same string into the seed.

Circle of life overlooked.

Is the life or principal part of the stem.

Is the first part that dies.

That in this part resides the principal *vitality* of the plant, I think I proved in my *former letter;* but I must add, that it is the *first part that dies*, when a branch is cut from a tree, or a tree torn up. In watching the fruit after a sudden frost, if taken *soon enough*, it is *this line* alone, that will appear to be *burnt*. In a few hours after, the rest of the pistil (at least the pointal and style) will be turned a reddish black; but after the first sign it never recovers. But in wood, if this line gets injured (either by the decay of the bud or other means) the circle will undulate into a thousand forms, to regain a wholesome situation in which to pursue its course. I have many curious specimens of decayed wood

wood rotted in this manner, that would explain this subject most evidently, and I have many drawings taken from other specimens, but too large to trouble Mr. Nicholson with; but which I may at a future time make public.

The circle of life found best in decayed wood.

I was once fortunate enough to see a tree cut down, that had been managed according to Mr. Forsyth's excellent method; and procuring some specimens of it, the new wood had begun to form in the middle, where the *pith* should have come, but wood grew instead; and the circle of life, making a large circuit, left a place in the new part *for the pith*. I shall give a sketch from some of my drawings, as it may better explain the nature of the circle of life, which after a certain course returned to the place in the new wood, it would have occupied in the old; as if it did not *venture* on the fresh formed wood, till it was solid and secure. In the rotten wood these vessels may be always traced by their turning *black*, or *darkened*; and in an infant plant (if the seed is boiled for dissection) by their dark colour; though often quite white when alive. I have now before me an Anson's apricot tree, which has the extraordinary property of losing one of its branches every year (I believe it is common to the species). In dissecting it I find near eight inches dead, all but a small piece of the bark and inner bark, which has given *liquid enough* to form a new *flush of leaves*, apparently since the wood has been entirely *dead* (for the wood is totally void of moisture, and must have been without life some time). This shows whence *the leaves proceed*, and that the only nourishment they got was from the *carbonic acid gas they absorbed*. It is true they appeared languishing and ill; still they showed fresh leaves. But it is most curious to see the struggle the circle of life has made to maintain its existence in the injured part, and when I cut it, it was wholly in the bark: but I never found any but delicate fruit trees able to support such stagnations in the wood, it kills our forest trees; or at least the limb that has it; though they have many other complaints, quite as bad as this palsy. I never see a defective limb or branch, without endeavouring to find its cause of decay by dissecting it. The cherry tree is very subject to this complaint, but I know no tree that better shows the line of life, though of the

Mr. Forsyth's new wood.

Plants can give leaves though near dying.

Circle of life struggles to maintain itself.

same

same colour as the pith, it is so very clear in its undulations.

Curious growth of the poa reptans.

But of all the plants which prove the powers of the circle of life, none perhaps equal the *grass* called *poa reptans*. It grew in a piece of swampy rubbish ground at the bottom of my garden. I had often measured seven or eight yards in length in the winter, perfectly dead; and yet in June, or the end of May, perceived life beginning to show itself at the farthest end from the stalk. Surprised at this, I the next spring chose two, much alike, dissected one of them *the whole way*, and found a collection of little vessels, which in thickness was not larger than a very *fine thread*. It had got half way the length of the grass, which was about three yards. Having merely opened the cover, I laid it down again, and the little vessels continued growing, till they got to the end of the length of grass. They then made a stop, and I perceived the grass began to thicken; and at the end nearest the roots, the dead part became inflated with juice, lost by degrees its dead appearance, got thickened *about the joints* within; and at last shot forth fresh leaves and *fresh roots*, from every joint. I have since watched it with the greatest care, and find it is the circle of life, that runs *thus*, *protected by the dead scale*. When it is stopped by the cover ceasing, it waits till the season permits the rest to grow. But it should teach more than this; it will show, that the dead matter may be *inflated* with a *living juice*, and live again, provided the *life* at bottom *is not extinguished;* and I have since seen this in many things, as in the hydrangia, where the stalks apparently die down, and are inflated again, or at least a *part* of them; and I doubt not a gardener must know many instances. The extreme delicacy of the circle of life is the cause of the double pith; the parts around it get injured, it starts on this account from its place, and gets farther into the wood; and if it has gone very far, instead of returning the pith begins to form near it, till two complete piths appear with the circle of life surrounding each on one side; or if any wood is formed between they will each complete its circle of life. I could give an innumerable number of additional proofs of the right these vessels have to be called the *circle of life*, or propagation, did I not fear to disgust

Dead vegetable matter may be revived.

Cause of the double pith.

disgust and tire my reader; but I may at a future time give the rest.

Pith.

The pith, which I shall now turn to, I esteem merely as a source of *moisture* to the rest of the plant when wanted: it stops with every flower bud, and begins again to grow as *soon* as the bud is past: it decreases as the strength and size of the tree increases; it is the only part of the tree, that has no *vessels* to contain liquor, for it is a net only, not a bundle of *cylinders*. It has been said, that it is composed of a great variety of figures, but this is a mistake: take it out *extremely thin*, and most piths will be found of one figure only. There are, however, a few different sorts; the net of the dogwood is very curious, and the pith of the juglans, and a few others differ in *form*. The size of the pith will form a tolerable division between the tree and shrub.

Growth of the root.

I have but little to say of the root, except that I look upon it to be wholly formed of the rind, much thickened, and perhaps a very little of the bark, but to be *without inner bark*, to have a quantity of wood, *no spiral vessels*, and hardly any pith. I searched in vain for the larger vessels of the inner bark, till it struck me, that the want of it was the reason of there never being a leaf on a root. In Devon this is a trial more easily made than in any other place; and I have repeatedly been assured, that roots were found with leaves, but it always turned out *to be a branch which crossed the root;* and I always found it so, on dissecting it, to try the truth of the assertion.

Each part of the stem has each its particular part of the flower.

I shall now close my letter with endeavouring to prove the truth of an observation made long ago by that *excellent observer* Linnæus, and since so absolutely denied by many: I mean, "that each part of the stem has, when it arrives near the flower stalk, its peculiar juice" for the formation of each part *of the flower;* that the bark produces the calyx of the flower; the inner bark the corolla; the wood the stamen; the circle of life the pistil: and that they all join in forming the fruit and seed. Willdenouw says, that, without having recourse to the *plant*, or to *dissection*, it is at *once possible* to show the folly of supposing, that each *particular* part of the plant should produce only one part of the

Each part of the stem has each its particular part of the flower.

the flower, and he directly adduces the *syngenesian* class, which contains the *very plants*, that (if he had dissected them) would have *proved the mistake* of his *argument*. But as all my opinions are formed on *dissection alone*, I have no theory to carry on, if I deduce from what I see in the microscope a false conclusion. I am very ready on conviction to give up the point; but as I reason from no other data than dissection, I would ask him these simple questions: why, if the nourishment of each part of the stem is not confined to each *different part of the flower*, does the whole arrangement of the stalk *alter*, the moment it gets to the flower stalk? why are there particular vessels, to confine and carry the juice to each peculiar part, if it was not of consequence, that this juice should touch no other places? for what purpose is the curious and artificial management in the bottom and top of a seed vessel, which enables the dissecter to say, " there are five divisions of little vessels proceeding from the wood, I know therefore (though I do not see it) that this must be a *pentandrian flower;* here is but one middle vessel proceeding from the *circle of life* (for the pith stops,) it is therefore of the order *pentandria monogynia:* here are five divisions of little vessels proceeding from the inner bark, it must therefore have *five petals?* This is a simple way of showing the truth, and may disgust, but it is *truth*, and *should not do so;* I ardently wish to convince; because I am convinced myself. Cut above or below the seed vessel of a lily, a violet, a tulip, and *conviction will* I THINK *certainly follow*. Why in cutting below or above the seed vessel of a syngenesian flower, can you directly tell whether it is superflua, æqualis, or segregata? Look at the bottom of the seed vessel of the sonchus; every *pin hole* of the vessel of the male is carried up by corresponding vessels in the outward cuticle of the seed: this I have proved in the solar microscope, (diminutive as it is) it is *thus carried up till* it meets and joins the *ligature* of *the males;* and the female *liquor* is protruded through the *inside* of the seed, and is perhaps one of the *strongest proofs* of the impregnation of the female. In the syngenesian class *(see Plate* IX*)* the delicacy of the vessels, which may be supposed too small for a liquid to flow through, must

must not impede the belief that it does so, when we consider the circulation of blood in the diminutive animal that torments the body of the flea and louse. I have seen the liquor run up with the utmost celerity through the upper cuticle of a very small seed of the syngenesian order, till it met the male and continued its course. It must be understood, that the juice from the corolla flows in the rest of the cuticle, and the largest vessels are those for the male liquor. The production of these little floscules, and the curious arrangement for the vessels, and for the nourishment of each separate part, is so wonderful, that I hardly know an object that has given me more delight than the contrivance manifested in them, or a sight more formed to strike with wonder, when seen in the microscope: and how wholly is this beautiful order of arrangement counteracted by a double flower; that is, by finding *none* of these *peculiar* vessels, but a general confusion, that seems to make a mixture of the whole! I never permit myself to form any opinion what the thing is to be, before I have dissected it: my opinions are wholly taken from what I do see, which on this subject I have here given. The person who doubts need only dissect a lily, a violet, or any flower, below the seed vessel or above it, and, I fancy, he will be satisfied.

The line of life enters into leaves that bear the flowers.

In detailing the reasons I had to believe that the circle of life formed the pistil, and that it is the life of the plant, or rather may be better compared to the *spinal marrow and brain* of the animal frame, I forgot one of the strongest proofs, which is, "that, though the circle of life never runs into any other leaf, it is to be found in all those *leaves that have the flower either on the middle* or on the side of the leaf, &c. I first to my great astonishment perceived it in the butcher's broom, where it leads directly up to the flower: then in scolopendrums, and afterward in the xylophyllas, &c. Besides that there is vastly more wood than in any other kind of leaves, as every one will feel on breaking them, the circle of life may easily be traced, as leading from one flower to the other. But I shall detain the reader no longer than to say, that these ideas and discoveries are not the hasty productions of momentary examination, but the result of many years of study; I may

The line of life enters into leaves that bear the flowers.

say *intense* study: though till now I have not had the courage to lay the result before the public, till I found, that my discoveries were likely to be *superseded* and *published* from the study of others: as I discovered, six years ago, the second organ in plants, which was so well explained in a paper in your excellent Journal, though rather too obscure. I have not mentioned the sensitive plants, because I have not *yet* completed my study of them; but I must observe, they so intirely *confirm* my idea, that " the motion of plants is caused almost wholly by the spiral nerves," that when I lay them before the public, they will I hope eradicate every *remaining doubt* that may be left in the mind: and that they are only more or less sensitive, as the length to which they are fastened is more or less extended.

DEAR SIR,

Your obliged Servant,

A. IBBETSON.

Belleveu, June 12th.

Explanation of the Figures.

Plate VIII, Fig. 7. Divisions of the wood in the stem of trees; *a*, the rind; *b*, the bark; *c*, the inner bark; *d*, the wood; *e*, the spiral nerves; *f*, the line of life; *g*, the pith; *h*, *h*, the silver grain; *o*, *o*, *o*, the bastard grain.

Fig. 8. Cylinders of the inner bark.

Fig. 9. Cylinders of the wood.

Plate IX. Fig. 10. Part of a branch showing the manner in which the line of life, *cc*, enters into the flower bud, *a*, and passes by the leaf buds *bb*; also the manner in which it runs to avoid an injured part.

Fig. 11. A flower bud, showing the line of life, *c c*, running up to each flower, *a*, *a*, *a*, *a*, *a*, *a*, *a*, and the pith terminating at *b*.

Fig. 12. A seed vessel of the class syngenesia; *a*, the calyx, *b*, female florets, *c*, male and female florets.

Fig. 13. Section just above the seed vessel of the dianthus. *a*, the calyx proceeding from the bark; *b*, the corolla, from the inner bark; *c*, *c*, *c*, *c*, *c*, ten stamens from the wood; *d*, the seed vessel; *e*, the pistil from the circle of life.

III. *On*

III.

On the supposed Perspiration of Plants. By Mrs. AGNES IBBETSON.

To Mr. NICHOLSON,

SIR,

A FRIEND has suggested to me, that, to avoid all mistakes, I should have described the various kinds of moisture, that might be taken for the perspiration of plants; lest the subject, *from their appearance*, should be given up as a dream of the author's, without a fair and candid trial. It is certainly worth it, for great must be its influence on the atmosphere, and *immense* the calculation of the water necessary to *afford such a perspiration*, if we take into account also the quantity wanted for *their growth*. But, I may say, if leaves exude, in proportion to their surface, as much moisture as a healthy man, they must often drop water in the driest days; which I never could perceive they did. But if (as is insisted on) they yield 17 times as much as a robust subject, every tree must be a shower bath, and we could not sit under one without a complete wetting *.

Moisture mistaken for perspiration should be described.

Perspiration ascribed to plants enormous.

Of the various appearances of moisture, which the solar microscope so completely elucidates, I shall first mention the honey dew, though there are few not acquainted with its appearance. *Beside this*, there *are three others*, one the bladder in which a small insect infolds its larva; another sort in which an insect lays her eggs; and the third is the sickness of a plant; for there are few plants, that do not give out a sort of sugar when ill. After these I must mention the egg of some insect. It is found on the proteas, and one or two other plants. I have preserved the eggs till the animals showed themselves. The next is the cryptogamia found on the pea, the sun flower leaf, the mimulus, and a few others; of these I have given a sketch, just as I took them from the solar microscope, that every one may judge whether this looks like the perspiration of a plant.

Different kinds of apparent moisture on plants.

* This does not follow, unless some cause were present, to condense the aqueous vapour perspired. C.

plant*. I have also seen the beginning of the hairs of the leaf taken for it.

No perspiration visible with a very high magnifying power.

In the three or four years that I have been (as long as the leaves last) endeavouring to discover perspiration, it appears to me impossible I should not have found it, *if it did exist:* but I have sought it with microscopes that magnified *so extremely*, as to prevent my being deceived by *other objects*. I regret indeed the little use made of an instrument now carried to a degree of perfection, which must daily bring new wonders to our admiring senses. With respect to perspiration, it is so little shown, though the smallest hairs of the leaf are enlarged to the size of a ruler, and the water is seen running up as the rarefaction of the air forces it from the increased warmth of the glass. Nay, the pores of the leaf are so enlarged, that an object five times as small could be seen and examined: why then should I not see moisture, *if it existed?*

Heat increases the cryptogamian plants on leaves, and causes unnatural secretions.

The melon.

The cucumber.

In my former papers (which were written a long time since,) I did not mention (because I was not fully aware of it) the very defective manner made use of to try the quantity of perspiration given by plants, and to evince its existence, till the desire of studying the effect of various degrees of heat on plants, made me a constant attendant on the hot-house, green house, hot walls, and glasses, &c. I then found, that any increase of heat helped greatly to *increase* the *number of cryptogamian* plants on those leaves, on which they were not *at all inclined* to grow; and that, beside this, they produced secretions unknown to the plant in its natural situation. The melon gives a very curious one, found on the edge of the leaf of the plant every morning: but, instead of covering the plant from all air, leave *it a little* by *raising the glass*, and the moisture *intirely ceases*. It is the same, though not so much, with the cucumber. There is not the smallest appearance of moisture without the plant is first rendered ill, to study its secretions. It is objected to me, that I left the plant so long (being three hours) that

* See Plate IX, Figs. 15, and 16, the cryptogamian plant on the mimulus, or monkeyflower: fig. 17, those on the pea, which are recumbent: fig. 18, those on the sunflower. They seldom appear on young leaves, or on any leaf, till the plant is near flowering.

the

the moisture under the glass had evaporated. It might perhaps have given a little more in a shorter time, and the hygrometer would have marked a trifle more moisture; but it is forced from the plant, and, so far from giving it naturally, I have every reason to believe, that it acts *as heat does*, and tears its way through the cuticle, as animals in an air pump will sometimes have the blood forced through the pores of the skin.

We cannot judge of the secretions of a plant in confinement. Mirbel.

It is certain, that a plant cannot exist without air, and that it languishes in a confined air. In this state how impossible to judge of its secretions. I cannot help being persuaded, that excellent botanist Mirbel had many doubts of its existence. The clear and simple account he gives of the production of the gasses and juices of plants *is such*, that *but for one line*, it would be the most perfect thing I ever saw; I hope I may be excused translating the few lines. "It is certain, that the carbonic acid gas, produced and renewed without ceasing by combustion, is dissolved in water, which the atmosphere holds suspended in vapour; and which passes through the thin cuticle of the leaves, and penetrates the albumen, and gains the nourishing vessels. This absorption takes place when the sap and other fluids (at first dilated by the heat of the day,) become condensed by the cold of the night, and fall towards the lower extremities of the tree; *for then* the liquids take *less room*, a sort of *vacuum takes place* in the higher parts; and the vapours flowing around enter the leaves by the pores, as we see water force itself into the pipe of a pump by the help of the piston, that produces a vacuum. But as soon as the sun appears above the horizon, these same fluids, joined to those the roots have pumped up from the earth, drawn by heat, *are carried into the leaves*, *and escape by the pores*, and it is then that the *water* and *carbonic acid* gas enforced by light are *decomposed*, and the torrent of oxigen flows from the leaves."

Water cannot be decomposed if it escape through the pores.

Now if the water escapes through the pores, how can it be there to be decomposed by the light, and to give out its oxigen? Setting aside therefore this line, it is the clearest account of vegetation, and the most just, I had ever the pleasure of reading. But certain it is, that, if plants per-

 spired,

spired, they could not give out oxigen. However, though the appearance of perspiration has invariably proved either a cryptogamian plant; the bubbles which hold the perfumed liquor of leaves, and which are to be found in all leaves that are scented; the eggs of insects; the edges of the pores, &c.; I do not deny, that there may be a very trifling degree of insensible perspiration: for I think that sort of scurf, or jelly, found on the leaves, arises from it; but this is trifling, and scarcely worth mentioning.

A trifling perspiration.

The vine perspires from its stalk.

Of the innumerable quantity of plants I have examined, there is but one, that in my opinion *really does perspire;* and that not on the leaf, but the *stalk. This is the vine.* When the vine is extremely full of juice, a bubble appears on the stalk, which, magnified, *is not a plant*; but really issues from the vine as the proper juice of it, for I can see no stalk. With the same truth I should have mentioned it, if I had found hundreds; for to attain truth is my aim, and I am really attached to no system whatever. Mine are merely desultory discoveries, *not mine indeed*, but those of the *solar microscope*, to which I transfer all the honour, if there is any. As to the sickness of a plant, any person may perceive, when a plant has been gathered an hour or two, how damp and moist it grows; it is the same when placed under a glass, it droops and grows clammy.

I am, Sir,

Your obliged Servant,

AGNES IBBETSON.

Belleveu 26th June.

IV.

A numerical Table of elective Attractions; with Remarks on the Sequences of double Decompositions. By THOMAS YOUNG, *M.D. For. Sec. R.S.* *

Attempts at numerical tables of elective attractions.

ATTEMPTS have been made, by several chemists, to obtain a series of numbers, capable of representing the mutual

* Philos. Trans. for 1809, Part I, p. 148. For a Memoria Technica of the double elective attractions, communicated by the learned author, see Journal, Vol. XXII, p. 304.

tual attractive forces of the component parts of different salts; but these attempts have hitherto been confined within narrow limits, and have indeed been so hastily abandoned, that some very important consequences, which necessarily follow from the general principle of a numerical representation, appear to have been entirely overlooked. It is not impossible, that there may be some cases, in which the presence of a fourth substance, beside the two ingredients of the salt, and the medium in which they are dissolved, may influence the precise force of their mutual attraction, either by affecting the solubility of the salt, or by some other unknown means, so that the number, naturally appropriate to the combination, may no longer correspond to its affections; but there is reason to think, that such cases are rare; and when they occur, they may easily be noticed as exceptions to the general rules. It appears therefore, that nearly all the phenomena of the mutual actions of a hundred different salts may be correctly represented by a hundred numbers, while, in the usual manner of relating every case as a different experiment, above two thousand separate articles would be required.

A series of numbers found, answering very generally.

Having been engaged in the collection of a few of the principal facts relating to chemistry and pharmacy, I was induced to attempt the investigation of a series of these numbers; and I have succeeded, not without some difficulty, in obtaining such as appear to agree sufficiently well with all the cases of double decompositions which are fully established, the exceptions not exceeding twenty, out of about twelve hundred cases enumerated by Fourcroy. The same numbers agree in general with the order of simple elective attractions, as usually laid down by chemical authors; but it was of so much less importance to accommodate them to these, that I have not been very solicitous to avoid a few inconsistencies in this respect; especially as many of the bases of the calculation remain uncertain, and as the common tables of simple elective attractions are certainly imperfect, if they are considered as indicating the order of the independent attractive forces of the substances concerned. Although it cannot be expected, that these numbers should be accurate measures of the forces which they represent, yet they may

Common tables of simple elective attractions imperfect.

be supposed to be tolerable approximations to such measures; at least if any two of them are nearly in the true proportion, it is probable, that the rest cannot deviate very far from it: thus, if the attractive force of the phosphoric acid for potash is about eight tenths of that of the sulfuric acid for barita, that of the phosphoric acid for barita must be about nine tenths as great; but they are calculated only to agree with a certain number of phenomena, and will probably require many alterations, as well as additions, when all other similar phenomena shall have been accurately investigated.

The facts may be represented independent of hypothesis.

There is, however, a method of representing the facts, which have served as the bases of the determination, independently of any hypothesis, and without being liable to the contingent necessity of any future alteration, in order to make room for the introduction of the affections of other substances; and this method enables us also to compare, upon general principles, a multitude of scattered phenomena, and to reject many which have been mentioned as probable, though doubtful, with the omission of a very few only, which have been stated as ascertained. This arrangement simply depends on the supposition, that the attractive force, which tends to unite any two substances, may always be represented by a certain constant quantity.

There must be a sequence in the simple attractions. Errours in the common tables.

From this principle it may be inferred, in the first place, that there must be a sequence in the simple elective attractions. For example, there must be an errour in the common tables of elective attractions, in which magnesia stands above ammonia under the sulfuric acid, and below it under the phosphoric, and the phosphoric acid stands above the sulfuric under magnesia, and below it under ammonia: since such an arrangement implies, that the order of the attractive forces is this; phosphate of magnesia, sulfate of magnesia, sulfate of ammonia, phosphate of ammonia, and again phosphate of magnesia; which forms a circle, and not a sequence. We must therefore either place magnesia above ammonia under the phosphoric acid, or the phosphoric acid below the sulfuric under magnesia; or we must abandon the principle of a numerical representation in this particular case.

In

In the second place, there must be an agreement between the simple and double elective attractions. Thus, if the fluoric acid stands above the nitric under barita, and below it under lime, the fluate of barita cannot decompose the nitrate of lime, since the previous attractions of these two salts are respectively greater, than the divellent attractions of the nitrate of barita and the fluate of lime. Probably, therefore, we ought to place the fluoric acid below the nitric under barita; and we may suppose, that, when the fluoric acid has appeared to form a precipitate with the nitrate of barita, there has been some fallacy in the experiment.

The simple and double attractions must agree.

The third proposition is somewhat less obvious, but perhaps of greater utility: there must be a continued sequence in the order of double elective attractions; that is, between any two acids, we may place the different bases in such an order, that any two salts, resulting from their union, shall always decompose each other, unless each acid be united to the base nearest to it: for example, sulfuric acid, barita, potass, soda, ammonia, strontia, magnesia, glycina, alumina, zirconia, lime, phosphoric acid. The sulfate of potass decomposes the phosphate of barita, because the difference of the attractions of barita for the sulfuric and phosphoric acids is greater than the difference of the similar attractions of potass; and in the same manner the difference of the attractions of potass is greater than that of the attractions of soda; consequently the difference of the attractions of barita must be much greater than that of the attractions of soda, and the sulfate of soda must decompose the phosphate of barita: and in the same manner it may be shown, that each base must preserve its relations of priority or posteriority to every other in the series. It is also obvious, that, for similar reasons, the acids may be arranged in a continued sequence between the different bases; and when all the decompositions of a certain number of salts have been investigated, we may form two corresponding tables, one of the sequences of the bases with the acids, and another of those of the acids with the different bases; and if either or both of the tables are imperfect, their deficiencies may often be supplied, and their errours corrected, by a repeated comparison with each other.

A continued sequence in the order of double attractions.

Correction of errours in tables.

In

Tables formed from cases collected by Fourcroy.

In forming tables of this kind from the cases collected by Fourcroy, I have been obliged to reject some facts, which were evidently contradictory to others, and these I have not thought it necessary to mention; a few, which are positively related, and which are only inconsistent with the principle of numerical representation, I have mentioned in notes but many others, which have been stated as merely probable, I have omitted without any notice. In the table of simple elective attractions, I have retained the usual order of the different substances; inserting again in parentheses such of them as require to be transposed, in order to avoid inconsequences in the simple attractions: I have attached to each combination marked with an asterisk the number deduced from the double decomposition, as expressive of its attractive force; and where the number is inconsistent with the corrected order of the simple elective attractions, I have also enclosed it in a parenthesis. Such an apparent inconsistency may perhaps in some cases be unavoidable, as it is possible, that the different proportions of the masses, concerned in the operations of simple and compound decomposition, may sometimes cause a real difference in the comparative magnitude of the attractive forces. Those numbers, to which no asterisk is affixed, are merely inserted by interpolation, and they can only be so far employed for determining the mutual actions of the salts to which they belong, as the results which they indicate would follow from the comparison of any other numbers, intermediate to the nearest of those, which are more correctly determined. I have not been able to obtain a sufficient number of facts relating to the metallic salts, to enable me to comprehend many of them in the tables.

Divisions of attractions.

It has been usual to distinguish the attractions, which produce the double decompositions of salts, into necessary and superfluous attractions; but the distinction is neither very accurate, nor very important: they might be still farther divided accordingly as two, three, or the whole of the four ingredients concerned are capable of simply decomposing the salt in which they are not contained; and if two, accordingly as they are previously united or separate: such divisions would however merely tend to divert the attention from the natural operation of the joint forces concerned.

 It

It appears to be not improbable, that the attractive force of any two substances might, in many cases, be expressed by the quotient of two numbers appropriate to the substances, or rather by the excess of that quotient above unity; thus the attractive force of many of the acids for the three principal alkalis might probably be correctly represented in this manner; and where the order of attractions is different, perhaps the addition of a second, or of a second and third quotient, derived from a different series of numbers, would afford an accurate determination of the relative force of attraction, which would always be the weaker, as the two substances concerned stood nearer to each other in these orders of nnmbers; so that, by affixing, to each simple substance, two, three, or at most four numbers only, its attractive powers might be expressed in the shortest and most general manner. Expression of the attractive force of two substances.

I have thought it necessary to make some alterations in the orthography generally adopted by chemists, not from a want of deference to their individual authority, but because it appears to me, that there are certain rules of etymology, which no modern author has a right to set aside. According to the orthography universally established throughout the language, without any material exceptions, our mode of writing Greek words is always borrowed from the Romans, whose alphabet we have adopted: thus the Greek vowel Υ, when alone, is always expressed in Latin and English by Y, and the Greek dipthong ΟΥ by U, the Romans having no such dipthong as OU or OY. The French have sometimes deviated from this rule, and if it were excusable for any, it would be for them, since their *u* and *ou* are pronounced exactly as the Υ and ΟΥ of the Greeks probably were: but we have no such excuse. Thus the French have used the term *acoustique*, which some English authors have converted into "acoustics;" our anatomists, however, speak, much more correctly, of the "acustic" nerve. Instead of glucine, we ought certainly, for a similar reason, to write glycine; or glycina, if the names of the earths are to end in *a*. Barytes, as a single Greek word, means weight, and must be pronounced bárytes; but as the name of a stone, accented on the second syllable, it must be written barites; and the pure earth may properly be called barita. Yttria I have altered to Itria, because no Latin word begins with a Y. Chemical orthography.

Table of the Sequences of the Bases with the different Acids.

In all mixtures of the aqueous solutions of two salts, each acid remains united to the base which stands nearest to it in this table.

Sulfuric Acid.

Barita	Barita	Barita	Barita	Barita	Barita	Potass	Barita	Barita	Lead
Strontia	Strontia	Potass	Potass	Potass	Potass	Soda	Strontia	Potass	Mercury
Lime	Lime	Soda	Soda	Soda	Soda	Barita	Lime	Soda	Iron
(Silver ?)	Potass	Ammonia	Strontia	Strontia	Strontia	Strontia	Potass	Ammonia	Potass
(Mercury ?)	Soda	Strontia	Ammonia	Ammonia (4)	Ammonia (5)	Ammonia (6)	Soda	Strontia	Soda
Potass	(Mercury ?)	Magnesia (3)	Magnesia	Magnesia (4)	Magnesia	Lime	Magnesia ?	Magnesia	Magnesia
Soda	(Iron ?)	Glycina	Glycina	Glycina	Lime	Magnesia	Ammonia	Glycina	
Zinc	Magnesia	Alumina	Alumina	Alumina	Glycina	Glycina	Glycina	Alumina	Lead
Iron	Ammonia (2)	Zirconia	Zirconia	Zirconia	Alumina	Alumina	Alumina	Zirconia	Zinc
Copper	Glycina	Lime	Lime	Lime	Zirconia	Zirconia	Zirconia	Lime?	Copper
Magnesia	Alumina (2)								
Ammonia (1)	Zirconia								
Glycina	(Copper ?)								
Alumina									
Zirconia									
Nitric	Muriatic	Phosphoric	Fluoric	Sulfurous	Boracic	Carbonic	(Nitrous)	(phosphorous)	(Acetic)

(1) Ammonia stands above magnesia when cold. (2) A triple salt is formed. (3) Perhaps magnesia ought to stand lower. (4) A compound salt is formed, and when hot, magnesia stands above ammonia. (5) Fourcroy says, that sulfate of strontia is decomposed by borate of ammonia. (6) With heat, ammonia stands below lime and magnesia.

Nitric Acid.	Nitric and Muriatic Acids.				
Barita	Potass	Barita	Potass	Barita (10)	Potass
Potass	Soda	Potass	Soda	Potass	Soda
Soda	Ammonia	Soda	Ammonia	Soda	Barita (10)
Strontia	Magnesia	Ammonia	Magnesia	Ammonia	Ammonia (7, 11)
Lime	Glycina	Magnesia	Glycina	Magnesia	Magnesia (7)
Magnesia (7)	Alumina	Glycina	Alumina	Glycina	Strontia
Ammonia (7)	Zirconia (8)	Alumina	Zirconia	Alumina	Lime
Glycina	Barita	Zirconia	Barita	Zirconia	Glycina
Alumina	Strontia	Strontia (9)	Strontia	Strontia	Alumina
Zirconia	Lime	Lime	Lime	Lime	Zirconia
Muriatic	Phosphoric	Fluoric	Sulfurous	Boracic	Carbonic

(7) A triple salt is formed. (8) Fourcroy says, that the muriate of zirconia decomposes the phosphates of barita and strontia. (9) According to Fourcroy's account, the fluate of strontia decomposes the muriates of ammonia, and of all the bases below it; but he says in another part of the same volume, that the fluate of strontia is an unknown salt. (10) According to Fourcroy's account of these combinations, barita should stand immediately below ammonia in both of these columns. (11) With heat, the carbonate of lime decomposes the muriate of ammonia.

Phosphoric Acid.

Barita	Lime	Barita	Potass	Barita
Lime	Barita	Lime	Soda	Lime
Potass	Potass	Potass	Barita	Potass
Soda	Soda	Soda	Lime (13)	Soda
Strontia	Strontia	Strontia	Strontia	Strontia
Magnesia	Magnesia	Ammonia (12)	Ammonia	Magnesia
Ammonia	Ammonia	Magnesia	Magnesia	Glycina ?
Glycina	Glycina	Glycina	Glycina	Alumina
Alumina	Alumina	Alumina	Alumina	Zirconia
Zirconia	Zirconia	Zirconia	Zirconia	
Fluoric	Sulfurous	Boracic	Carbonic	(phosphorous)

(12) According to Fourcroy, the phosphate of ammonia decomposes the borate of magnesia. (13) Fourcroy says, that the carbonate of lime decomposes the phosphates of potash and of soda.

Fluoric Acid.

Lime	Lime	Potass
Potass	Barita	Soda
Soda	Strontia	Lime
Magnesia	Potass	Barita
Ammonia	Soda	Strontia
Glycina	Ammonia	Ammonia (14)
Alumina	Magnesia	Magnesia
Zirconia	Glycina	Glycina
Strontia	Alumina	Alumina
Barita	Zirconia	Zirconia
Sulfurous	Boracic	Carbonic

(14) According to Fourcroy, the carbonate of ammonia decomposes the fluates of barita and strontia.

Sulfurous Acid.				Boracic Acid.
Barita	Potass	Lime	Zirconia	Potass
Strontia	Soda	Strontia	Alumina	Soda
Potass	Barita (15)	Barita	Glycina	Lime
Soda	Strontia	Zirconia	Ammonia	Barita
Ammonia	Ammonia	Alumina	Magnesia	Strontia
Magnesia	Lime	Glycina	Strontia	Magnesia
Lime	Magnesia	Magnesia	Soda	Ammonia
Glycina	Glycina	Ammonia	Potass	Glycina
Alumina	Alumina	Soda	Barita	Alumina
Zirconia	Zirconia	Potass	Lime	Zirconia
Boracic	Carbonic	(Nitrous)	(Phosphorous?)	Carbonic

(15) Fourcroy says, that the sulfite of barita decomposes the carbonate of ammonia.

Table

Table of the Sequences of the Acids with different Bases.

	Barita.			Strontia.					Lime.				Potass		Magnesia.
													Soda		
Sulfuric	S	C	S	S	C	S	P	S	C	P	P	P	Magnesia=Ammonia	S	B
Nitric	N	S	P	N	SS	P	S	P	P	F	F	F	Glycina	N	C
Muriatic	M	P	SS	M	F	SS	SS	SS	F	B	B	SS	Alumina	M	P
Phosphoric	SS	SS	N	SS	P	F	F	F	B	SS	C	S	Zirconia	P	F
Sulfurous	P	N	M	C	B	B	B	B	SS	S	SS	B	Each with every subsequent base in this order.	F	SS
Fluoric	C	M	F	B	S	C	C	N	S	C	S	N		SS	S
Boracic	B	F	B	F	M	N	N	M	M	N	N	M		B	N
Carbonic	F	B	C	P	N	M	M	C	N	M	M	C		C	M
Strontia	LM	PT	MG	LM	PT	MG	AM	GL	PT	MG	AM	GL			AM
		SD	AM		SD			AL	SD			AL			
			GL					ZR				ZR			
			AL												
			ZR												

Comparative use of this table.

The comparative use of this table may be understood from an example: if we suppose, that the nitrate of barita decomposes the borate of ammonia, we must place the boracic acid above the nitric, between barita and ammonia in this table, and consequently barita below ammonia, between the fluoric and boracic in the former: hence the boracic and fluoric acids must also be transposed between barita and strontia, and between barita and potass; or if we place the fluoric still higher than the boracic in the first instance, we must place barita below ammonia between the nitric and fluoric acids, where indeed it is not impossible that it ought to stand.

Numerical Table of elective Attractions.

Barita.		Strontia.		Potass.		Soda.	Lime.	
Sulfuric acid	1000*	Sulfuric acid	903*	Sulfuric acid			Oxalic acid	960
Oxalic	950	Phosphoric	827*		894*	885*	Sulfuric	868*
Succinic	930	Oxalic	825	Nitric	812*	804*	Tartaric	867
Fluoric		Tartaric	757	Muriatic	804*	797*	Succinic	866
Phosphoric	906*	*Fluoric*		Phosphoric			Phosphoric	865*
Mucic	900	Nitric	754*		801*	795*	Mucic	860
Nitric	849*	Muriatic	748*	Suberic?	745	740	Nitric	741*
Muriatic	840*	(Succinic)	740	Fluoric	671*	666*	Muriatic	736*
Suberic	800	(Fluoric)	703*	Oxalic	650	645	Suberic	735
Citric		*Succinic*		Tartaric	616	611	Fluoric	734*
Tartaric	760	Citric?	618	Arsenic	614	609	Arsenic	$733\frac{3}{4}$
Arsenic	$733\frac{1}{2}$	Lactic	603	Succinic	612	607	*Lactic*	732
(Citric)	730	*Sulfurous*	527*	Citric	610	605	Citric	731
Lactic	729	*Acetic*		Lactic	609	604	Malic	700
(Fluoric)	706*	*Arsenic*	($733\frac{1}{4}$)	Benzoic	608	603	Benzoic	590
Benzoic	597	Boracic	513*	Sulfurous	488*	484*	*Acetic*	
Acetic	594	(Acetic)	480	Acetic	486	482	Boracic	537*
Boracic	(515)*	Nitrous?	430	Mucic	484	480	Sulfurous	516*
Sulfurous	592*	Carbonic	419*	Boracic	482*	479*	(Acetic)	470
Nitrous	450			Nitrous	440	437	Nitrous	425
Carbonic	420*			Carbonic	306*	304*	*Carbonic*	423*
Prussic	400			Prussic	300	298	Prussic	290

Magnesia.		Ammonia.		Glycina?		Alumina.	Zirconia?
Oxalic acid	820	Sulfuric acid	808*	Sulfuric acid	718*	709*	700*
Phosphoric		Nitric	731*	Nitric	642*	634*	626*
Sulfuric	810*	Muriatic	729*	Muriatic	639*	632*	625*
(Phosphoric)	736*	Phosphoric	728*	Oxalic	600	594	588
Fluoric		Suberic?	720	Arsenic	580	575	570
Arsenic	733	Fluoric	613*	Suberic?	535	530	525
Mucic	$732\frac{1}{2}$	Oxalic	611	Fluoric	534*	529*	524*
Succinic	$732\frac{1}{4}$	Tartaric	609	Tartaric	520	515	510
Nitric	732*	Arsenic	607	Succinic	510	505	500
Muriatic	728*	Succinic	605	Mucic	425	420	415
Suberic?	700	Citric	603	Citric	415	410	405
(Fluoric)	620*	Lactic	601	*Phosphoric*	(648)*	(642)*	(636)*
Tartaric	618	Benzoic	599	Lactic	410	405	400
Citric	615	Sulfurous	433	Benzoic	400	395	390
Malic?	600?	Acetic	432	Acetic	395	391	387
Lactic	575	Mucic	431*	Boracic	388*	385*	382*
Benzoic	560	Boracic	430*	Sulfurous	355*	351*	347*
Acetic		Nitrous	400	Nitrous	340	336	332
Boracic	459*	Carbonic	339	Carbonic	325*	323*	321*
Sulfurous	439*	Prussic	270	Prussic	260	258	256
(Acetic)	430						
Nitrous	410						
Carbonic	366*						
Prussic	280						

Acids.

Acids.

Sulfuric.		Nitric.		Muriatic.		Phosphoric.	
Barita	1000*	Barita	849*	Barita	840*	Barita	906*
Strontia	903*	Potass	812*	Potass	804*	Strontia	827*
Potass	894*	Soda	804*	Soda	797*	*Lime*	(865)*
Soda	885*	Strontia	754*	Strontia	748*	Potass	801*
Lime	868*	Lime	741*	Lime	736*	Soda	795*
Magnesia	810*	Magnesia	732*	Ammonia	729*	*Ammonia*	(728)*
Ammonia	808*	Ammonia	731*	Magnesia	728*	Magnesia	736*
Glycina	718*	Glycina	642*	Glycina	639*	Glycina	648*
Itria	712	Alumina	634*	Alumina	632*	Alumina	642*
Alumina	709*	Zirconia	626*	Zirconia	625*	Zirconia	636*
Zirconia	700*						

Fluoric.		Oxalic.		Tartaric.	Arsenic.		Tungstic.
Lime	734*	Lime	960	867	Lime	733¾	Lime
Barita	706*	Barita	950	760	Barita	733½	Barita
Strontia	703*	Strontia	825	757	Strontia	733¼	Strontia
Magnesia	(620)*	Magnesia	820	618	Magnesia	733	Magnesia
Potass	671*	Potass	650	616	Potass	614	Potass
Soda	666*	Soda	645	611	Soda	609	Soda
Ammonia	613*	Ammonia	611	609	Ammonia	607	Ammonia
Glycina	534*	Glycina?	600	520	Glycina	580	Glycina
Alumina	529*	Alumina	594	515	Alumina	575	Alumina
Zirconia	524*	Zirconia?	588	510	Zirconia	570	Zirconia

Succinic.		Suberic.		Camphoric.	Citric.	
Barita	930	Barita	800	Lime	Lime	731
Lime	866	Potass	745	Potass	Barita	730
Strontia?	740	Soda	740	Soda	Strontia	618
(Magnesia)	732¼	Lime	735	Barita	Magnesia	615
Potass	612	Ammonia	720	Ammonia	Potass	610
Soda	607	Magnesia	700	Glycina?	Soda	605
Ammonia	605	Glycina?	535?	Alumina	Ammonia	603
Magnesia		Alumina	530	Zirconia?	Glycina?	415?
Glycina?	510	Zirconia?	525?	Magnesia	Alumina	410
Alumina	505				Zirconia	405
Zirconia?	500					

Lactic.		Benzoic.		Sulfurous.		Acetic.	
Barita	729	White oxid of arsenic		Barita	592 *	Barita	594
Potass	609	Potass	608	Lime	516 *	Potass	486
Soda	604	Soda	603	Potass	488 *	Soda	482
Strontia	603	Ammonia	599	Soda	484 *	*Strontia*	480
Lime	(732)	Barita	597	*Strontia*	(527) *	*Lime*	470
Ammonia	601	Lime	590	Magnesia	439 *	Ammonia	432
Magnesia	575	Magnesia	560	Ammonia	433 *	Magnesia	430
Metallic oxids		Glycina?	400?	Glycina	355 *	Metallic oxids	
Glycina	410	Alumina	395	Alumina	351 *	Glycina	395
Alumina	405	Zirconia?	390?	Zirconia	347 *	Alumina	391
Zirconia	400					Zirconia	387

Mucic?

Mucic?		Boracic.		Nitrous?		Phosphorous.
Barita	900	Lime	537 *	Barita	450	Lime
Lime	860	Barita	515 *	Potass	440	Barita
Potass	484	Strontia	513 *	Soda	437	Strontia
Soda	480	*Magnesia*	(459) *	Strontia	430	Potass
Ammonia	431	Potass	482 *	Lime	425	Soda
Glycina	425	Soda	479 *	Magnesia	410	Magnesia?
Alumina	420	Ammonia	430 *	Ammonia	400	Ammonia
Zirconia	415	Glycina	388 *	Glycina	340	Glycina
		Alumina	385 *	Alumina	336	Alumina
		Zirconia	382 *	Zirconia	332	Zirconia

Carbonic.		Prussic.	
Barita	420 *	Barita	400
Strontia	419 *	Strontia	
Lime	(423) *	Potass	300
Potass?	306 *	Soda	298
Soda	304 *	Lime	290
Magnesia	(366) *	Magnesia	280
Ammonia	339 *	Ammonia	270
Glycina	325 *	Glycina?	260
Alumina	323 *	Alumina?	258
Zirconia	321 *	Zirconia?	256

V.

Experiments on Sulphur and its Decomposition; by Mr. CURAUDAU, *Professor of Chemistry applicable to the Arts, and Member of several learned Societies* *.

WHEN bodies we attempt to decompose have experienced no alteration from the chemical agents, to the action of which they have been subjected, we are obliged to class them as simple bodies. The idea of simple substances, however, though there must be such, is but little reconcilable with the different phenomena of decomposition and recomposition, which nature is incessantly producing before our eyes, and I have never considered as simple all that are generally deemed so. On the contrary I have always thought, that the substances constituting the mineral kingdom, of whatever kind, are compounds; and that the principles of which

Bodies supposed simple.

None in the mineral kingdom.

* Journal de Physique, July, 1808, p. 12. Mr. Davy's decomposition of sulphur by the Voltaic pile is given at p. 321, of our present number.

which they are composed are the same, as those that enter into the composition of substances, that belong to the vegetable and animal kingdoms. But let me not be mistaken. The state in which we are acquainted with certain principles is very far from the great condensation they must experience, before they enter into the composition of the mineral kingdom. Accordingly the compounds of those that result from a union of these principles must differ, in proportion as they recede from the former state, or approach the latter. This in fact we observe in the vegetable kingdom. The essential oils, for example, must be considered as compounds, in which the principles are very near the gaseous state; while the elements that constitute the resins and fixed oils are in a state of the greatest condensation, with respect to the kingdom to which they belong. But this greatest condensation of the principles, that form the different compounds of the vegetable kingdom, is far removed from the first degree of condensation of the elements that constitute the substances of the mineral kingdom. Accordingly the indestructibility of the latter seems connected with the difficulty of causing principles to retrograde towards a state of less condensation, that have the very opposite tendency.

In which the elementary matters are greatly condensed.

In the vegetable kingdom they are less so.

Indestructibility of mineral bodies.

What I have just said of the different degrees of condensation, in which the principles that constitute all natural bodies exist, I advanced ten years ago in the first paper I had the honour to present to the Institute on the composition of alkalis: and I have seen with pleasure, that Mr. Berthollet, in adopting this opinion in his Chemical Statics, has taken it out of the rank of hypotheses.

This property, owing to the powerful affinity of their principles, merits consideration.

As to the indestructibility of mineral substances, to which I ascribe the difficulty of causing the principles that constitute them to retrograde toward a state of less condensation, this too is an opinion, which appears to me to merit all the attention of chemists. In fact, what power, except that of the mutual attraction of the principles that compose all the substances of the mineral kingdom, can enable them to resist the eminently dilatable action of caloric? Thus fire, to effect the decomposition of mineral substances,

substances, must be employed as an auxiliary, and not as an immediate agent.

The decomposition of sulphur, which constitutes the object of this paper, will furnish an application of the principle I have just laid down. However, before we attempt the decomposition of a substance, it is requisite, to have some notion of its composition, that may indicate the nature of the experiments to be made. With respect to sulphur for instance, I had observed, that sulphuric acid strongly saturated with nitrous gas gave a blue colour to water acidulated with it. From the appearance of this colour I inferred, that carbon must be one of the component parts of sulphur: and then considering the property this substance has to dissolve in oils, I suspected, that sulphur might be a compound of carbon and hidrogen. These conjectures were very far from a demonstration; but from these I could proceed as data, either to attack the principles themselves, or to combine them with a third principle, which by its union with them would form a compound already known.

Decomposition of sulphur, as of other bodies, proceeded on by induction.

Nitrogen, for example, appeared to me well adapted to give rise to the compound I should wish to obtain, if hidrogen and carbon were component parts of sulphur.

In fact, from a combination of these two principles with nitrogen must not a compound be produced analogous to the prussic radical? and would not this product, the elements of which are known, indicate those of sulphur?

Nitrogen should produce with it something like the prussic radical.

To verify how far my conjectures were well founded, I made the following experiment.

Experiment to prove this.

I subjected to calcination in an iron tube four parts of animal charcoal with two parts of sulphate of potash, the whole being intimately mixed. I heated this mixture to a cherry red, and having suffered it to cool to three fourths, I threw it into a large quantity of water.

Animal charcoal and sulphate of potash calcined, and lixiviated.

When I had filtered the liquor, it was of a green colour, inclining to blue according to the light in which it was viewed. It had but a slight smell of hidrosulphuret. Its taste, though different from that of the prussic radical, produced on the palate a sensation resembling that, by which this radical is characterised.

The lixivium

I tried

not precipitated by acids,

I tried afterward whether acids would precipitate sulphur from it, but even the oximuriatic scarcely rendered it turbid. They only evolved from it a peculiar smell, insupportably fetid. However as the nature of the solution indicated the presence of sulphur, I was willing to ascertain, whether it contained any. With this view I let fall into it a few drops of a solution of sulphate of iron at a maximum of oxidation, which immediately occasioned a black precipitate, that was speedily changed to blue by an additional quantity of the solution of the sulphate.

but blue with sulphate of iron.

The sulphur had formed a compound analogous to the prussic radical.

From these different experiments, and particularly from the property of the solution, I no longer doubted, that the sulphur had entered into combination with the nitrogen, and formed a compound analogous to the prussic radical.

Sulphuric acid with nitrous gas precipitated sulphur.

Having afterward examined, what action sulphuric acid saturated with nitrous gas would have on this solution, I remarked, that this acid produced a copious yellow precipitate in it, which to the eye had all the appearance of sulphur, and emitted a similar smell when thrown on live coals. This solution, like those before examined with acids, contained the prussic radical; and the precipitate here mentioned was nothing but this radical, which at the moment of its formation might be converted into Prussian blue by combining it with a few drops of solution of sulphate of iron.

This substance analogous to the prussic radical.

This compound then clearly indicates a substance analogous to the prussic radical, but differing from it in being more fixed, since the strongest acids do not separate it from its solution, while all of them readily decompose the prussiate of potash. Were this the only property, that characterised the radical of which I am speaking, it would be sufficient, to distinguish it from the prussic.

Its fixedness.

With regard to the great degree of fixedness of this new radical, it may be ascribed to the hidrogen, the condensation of which appears to be as strong in this compound, as it is in sulphur; a condensation however, which nitrogen can diminish in forming ammonia with the hidrogen by the decomposition of prussiate of iron.

Is carbon or hidrogen pre-

As to the question, whether carbon or hidrogen be the predominent principle in sulphur, it is obvious, that the process

process I employed to decompose it affords little means of finding the proportions of the two principles.

dominant in sulphur?

There is one observation however, that may throw some light on this question. I have remarked, that the solutions of sulphuretted nitrogen of potash [*azote sulfüré de potasse*] all contain an excess of carbon, which they let fall, if the liquor remain exposed to the open air: whence I have inferred, that the nitrogen did not find in the sulphur the proportion of carbon necessary for the formation of the prussic radical.

Probably hidrogen.

In the next paper I shall have the honour of communicating to the Institute I shall make known the elements of phosphorus, and of iron. I shall likewise notice in it the alkaline metals, in which it is said there is no carbon.

Future researches.

VI.

Experiments in Continuation of those on the Decomposition of Sulphur; by the Same.*

HAVING been informed, that the experiments related in my paper on the decomposition of sulphur have not appeared sufficiently decisive, to authorize the conclusion I have drawn from them, I am impatient to make known fresh facts, that may serve to confirm the results I obtained.

Experiments thought inconclusive.

Exp. 1. Instead of lixiviating the residuum of the calcination of animal charcoal and sulphate of potash, as was mentioned in my paper on sulphur, let it be intimately mixed with one fifth of sulphur, very dry and well levigated; and heat the mixture, either in a gunbarrel or in a stone retort. If the gasses produced in this operation be collected, it will be found, that a great deal of ammoniacal gas is evolved from the commencement of the experiment, to which will succeed hidrogen gas, and carburetted hidrogen gas. When nothing more is given out, extinguish the fire, and, as soon as the vessel is cold, lixiviate the matter it contains in about ten times its weight of water,

Principles of sulphur combined with nitrogen form the prussic radical.

* Journal de Physique, August 1808, p. 117.

and then filter. This lixivium differs from the former in being of a deeper colour, which announces, that carbon is dissolved in it in a larger proportion. It differs from it likewise in containing but little of the prussic radical. However, if it remain a few months in contact with the air, it will acquire more and more the property of precipitating the solution of sulphate of iron of a blue colour; which shows, that the principles of sulphur combined with nitrogen are capable of forming the prussic radical.

Remarkable phenomena

But what is particularly remarkable in this experiment is the hidrogen produced during the operation; also the carbon, which is dissolved in a large quantity in the lixivium; and lastly the almost total destruction of the prussic radical.

accounted for.

In the first place the hidrogen disengaged from a mixture, which gave out none previous to the addition of the sulphur, must necessarily be a product of the latter substance. In the second place, the carbon dissolved in the lixivium must likewise have belonged to the sulphur, since this is the only substance added to the mixture. And lastly the almost total destruction of the prussic radical is explicable by the presence of hidrogen in the sulphur, which, combining with the nitrogen, produces ammonia, that soon escapes from the mixture by iss volatility.

2d experiment.

Exp. 2. Solution of azotized sulphuret of potash acidulated with sulphuric acid, when mixed with a sufficient quantity of sulphate of iron at a maximum of oxidation, yields from a fourth to a third more prussian blue, than the same solution would give if acidulated with sulphuric acid saturated with nitrous gas.

Not justly explained.

Such a difference in the results could not fail to engage my attention, since, from the hypothesis of the disoxigenation of nitrous gas, this, instead of diminishing the proportion of prussian blue, on the contrary should have increased it. I judged from this, that the explanation, which had been given of the phenomenon in question, was not accurate; and that it must result from some other cause, than that on which it had been said to depend.

To ascertain how far this conjecture was well founded, I made

I made several experiments, among which the following appeared to me the most conclusive.

Exp. 3. The solution of azotized sulphuret of potash strongly acidulated with sulphuric acid saturated with nitrous gas yields a copious precipitate of sulphur, while all the other acids scarcely throw down any. 3d experiment.

Several chemists, to explain this truly remarkable property of nitrous gas, have supposed, that this gas was decomposed; and that its oxigen, by combining with the hidrogen that holds the sulphur in solution, favours the precipitation of the sulphur. Explained

Yet if it were true, that oxigen had the property of precipitating sulphur from its solution, why does not the oximuriatic acid act in the same manner as the nitrous gas? Can oxigen possess two such opposite properties, particularly when it acts in similar circumstances? This explanation then presents an anomaly far from favourable to the different hypotheses opposed to the consequences I have drawn from my experiments. It is proper therefore to examine the question in another point of view. erroneously.

In the first place nitrous gas does not act in the solution of azotized sulphuret of potash by oxigenizing the hidrogen of the sulphuret: for this solution, far from containing a surplus of hidrogen beyond the composition of the sulphur, is on the contrary deprived of a part of that which constitutes the sulphur. Accordingly it is by hidrogenizing the dishidrogenized carbon of the sulphur, that the latter is precipitated from its solution, which is very different from the explanation that has been given of this phenomenon. Thus the nitrous gas acts on the solution of azotized sulphuret of potash only in consequence of the affinity this gas has for oxigen, and of that which the dishidrogenized carbon of the sulphur has for hidrogen; an action that concurs at the same time to decompose the water, and with which is combined that exerted by the sulphur on the oxigen. accounted for.

VII. *On*

VII.

On the Camera Lucida. In a Letter from Mr. T. Sheldrake.

To Mr. NICHOLSON,

SIR,

Camera lucida. HAVING been much pleased with the description of the Camera Lucida in your 70th Number, I procured one of the instruments, and made experiments to ascertain the extent of its merits when compared with those of the Camera Obscura. I beg leave to send the result of these experiments, for the information of your readers in general, and in hopes that they may induce the ingenious inventor of the Camera Lucida to bring it still nearer to perfection.

Defects of the camera obscura. The defects of the camera obscura are, that it is cumbersome to carry about and set up for use; that the objects it reflects are, under some circumstances, deficient in point of brilliancy, and that the objects are, under some circumstances, a little distorted from the truth of perspective. For these defects, the skilful artist, who chooses to make use of the instrument, will know how to provide a proper remedy. The drawings that are said to have been made by Abyssinian Bruce* by the assistance of this instrument, the

Bruce a good draughtsman. * It was once fashionable to accuse Mr. Bruce of every kind of breach of veracity: among other things it was said, that he could not draw, and that the drawings he showed as his own were not his, but made by another person. Time has done him justice in many particulars, and if any one still believes that which was said of his drawings, I may, perhaps, contribute a mite towards doing him justice on that head.

Between twenty and thirty years ago there was a sale of drawings at Hutchins's Rooms, King Street, Covent Garden, among them were many drawings, some finished, and others only sketches, which the Auctioneer publicly declared at the sale to have been made by a Mr. Bruce, who had been on a public mission to one of the States of Barbary, and was *then absent on a journey to Abyssinia.*

My father purchased some of these drawings, so that I had them several

the drawings that were certainly made by Mr. Daniel by means of this instrument, and the drawings which are said to be made for different panoramas by the same means, afford convincing proofs, that it may be of great practical utility in delineating objects with truth and *facility*, greatly superior to what can be practiced even by eminent artists without its assistance. Drawings made by it.

The great advantage of the camera obscura is, that it fixes the objects to be represented upon the surface, so that when the artist has taken his station, and arranged his instrument, he has nothing to do but run his pencil over the objects which he sees lie under his hand, and, in proportion to his capacity for drawing with correctness and facility the objects which lie before him, will his drawings be masterly, beautiful, and correct. What advantages has the camera lucida to oppose to the disadvantages of the camera obscura, or to put in competition with the advantages which the latter instrument is known to possess? Its advantages.

The camera lucida is portable in a very small compass; it represents objects with more brilliancy and distinctness than the camera obscura; and it represents them either singly or in combination, with perfect truth and correctness of perspective. What disadvantages has it then to counterbalance these particulars in which it is evidently superior, in a very great degree, to the camera obscura? Advantages of the camera lucida.

This will, perhaps, be best illustrated by referring to the annexed sketch from nature, which I have drawn with the naked eye; which I attempted to draw with the camera lucida, but could not, and which I have no doubt that I could have drawn with more correctness, facility, and expedition in the camera obscura, than in any other manner. Its disadvantages

several years under my eye; they consisted of figures drawn from nature in the fashionable dress of the time, the sketches drawn with much truth and spirit, the finished drawings tinted with so much taste, that I have no doubt the hand that made them was equial to any thing that was afterward produced as Bruce's, and as they were publicly sold as his before he had acquired any public reputation, or excited the tongue of envy to injure him, there is every reason to believe, that they were actually drawn by Mr. Bruce. These drawings were favourites with me so long as I had access to them; but my father's collection was sold after his death, and I know not what became of them.

When

instanced.

When I had taken my stand, arranged my paper, and fixed the camera lucida upon it, I had, upon looking into the eye glass, a distinct view of the whole scene, as perfect as the instrument would represent it; but a different arrangement was necessary, before I could have a *chance* of copying, or, if you please, drawing it: I was to alter the position of the eye glass, so that I should, in the upper part of it, see such of the objects as I was to imitate; and, in the lower part, a distinct representation of the paper and pencil with and upon which I was to draw; these two divisions will admit of different proportions, but, to speak in general terms, we may say, the upper part contains a correct view of part of the objects that are to be drawn, the lower part contains a correct view of the paper on which they are to be drawn, and the pencil by which the drawing is to be made: the operation to be performed is, to look upon the representation of the objects, and the representation of the pencil and paper at the same moment, and to copy exactly upon the lower, what is seen upon the upper part of the object glass; this every man will do in proportion to the power he has of imitating the forms of objects that are placed before him. The essential difference between the camera obscura and the camera lucida is, that the former fixes upon the paper the whole of the picture at one view, and the artist has only to pass his pencil over it to render it permanent, which he has the power to do with more correctness and expedition, and equal facility, as if he was drawing without the use of the instrument. The camera lucida, on the contrary, places before the eye a certain portion of the objects to be imitated, and a certain portion of the paper on which the imitation is to be drawn: the difference between the two operations will be exactly as the difference between tracing and drawing against the window, and copying the same drawing if placed before you upon the table: this is the difference upon a view of the whole proceeding, but, upon descending to minutiæ, other circumstances bear still more against the camera lucida.

Difference between the two.

The process farther described.

The circle *Fig.* 2, Pl. X, contains a representation of so much of the view as can be seen at the same time, with so much of

of the paper on which, and the pencil by which it is to be imitated: of course the draughtsman will copy correctly on the lower part those objects which he sees in the upper part of the glass; but these objects constitute but a small part of the whole view; if the remainder is to be attained it must be with great trouble and difficulty: it is true that by moving my head to one side, and looking diagonally across the eye glass, I could see objects that were not visible upon looking directly into it, and thus by moving my head from one side to the other I could get all the horizontal lines, and those lines which approach to the horizontal position upon the paper, so that by this method I could get all the horizontal lines that were within the range of the instrument or the drawing: but it was impossible, by any artifice to do as much with the perpendicular lines, or those which approach to the perpendicular direction, without altering the position of the glass, and in doing this it was found impossible to connect the different portions of the scene that were viewed upon changing the position of the glass, with a degree of truth comparable to what may be attained by the camera obscura without any trouble at all.

The process farther described.

The reader will perhaps comprehend the difficulty if he imagines the great tree in the foreground to be divided horizontally into four or more parts, each of which must be seen by itself and drawn by itself: the glass must then be shifted so as to see and draw another portion without seeing that which had first been drawn, and so on till the whole was completed. Independent of the trouble and waste of time that would be necessary to shift the glass, if it could be done with accuracy, the circumstance of not being able to see the whole of the scene while one is drawing it, and of course comparing the effect of the whole is extremely unpleasant: the instrument must be removed from the paper before the effect of the drawing could be seen, and if it should be necessary to correct it, it is next to impossible to replace it with sufficient accuracy to avoid making false lines, and of course destroying the truth of the representation.

I have stated the inconveniencies that *I* have found, in making

Method of obviating the

inconveniences desirable.

making use of this instrument, and for which I could not find a remedy; others have, as I am informed, found the same inconveniences, and not been able to obviate them; some may have been more fortunate; and if they have, they will render a very acceptable service by pointing out the means of removing these defects: but, if they should do so, I believe it will still be impossible to produce a view, of any magnitude, by means of the camera lucida, with as much ease, expedition, and in as masterly a manner as an able artist can, if he pleases, draw in the camera obscura. This opinion I must entertain, till I see drawings as masterly in point of execution as Mr. Daniel's views in India, made by means of the camera lucida; I mention Mr. Daniel's views on this occasion, because I have been credibly informed, that they were all drawn in the camera obscura, and, as they are well known, they form a good public standard of comparison.

Mr. Daniel's views.

An instrument for taking views still desirable.

It appears then, that a perfect instrument to be used as a delineator is still a desideratum, and will be obtained when the separate advantages of the camera obscura and the camera lucida can be united in the same instrument, and not be diminished by any of the inconveniences to which each of them is at present subject.

I am, Sir,

Your most obliged Servant,

T. SHELDRAKE.

50, *Strand*,
July 6th, 1809.

References to the Drawing.

Fig. 1. Sketch from nature as it may be seen and drawn immediately in the camera obscura.

Fig. 2. Part of the same view as seen in the camera lucida; the upper half contains a portion of the horizontal lines in the view as reflected in the glass: the lower half shows the pencil imitating the same lines upon the paper, it is obvious that by looking diagonally into the glass the view may be extended so as to take in a portion of those lines which cannot be seen when looking directly into the glass.

Fig. 3. Part of the tree seen in the upper half reflected in

Nicholson's Philos: Journal, Vol. XXIII. Pl. X. p. 376.

Mr. T. Sheldrake on the Camera Lucida.

Fig. 1.

Fig. 2.

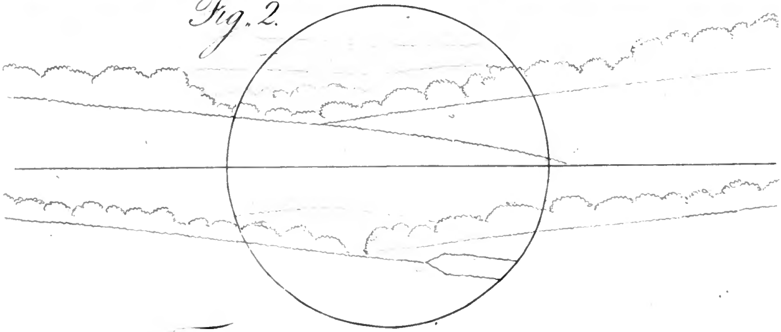
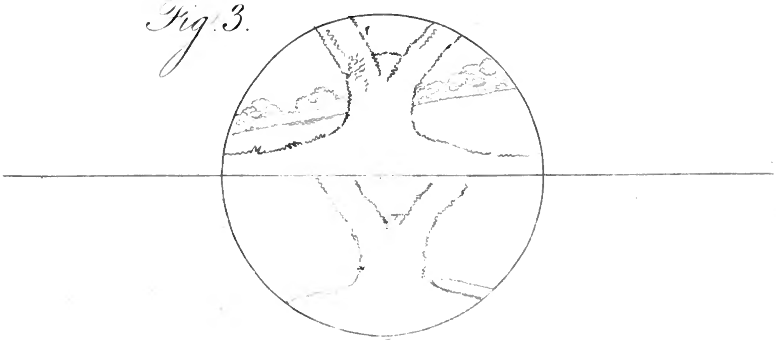


Fig. 3.



in the glass, the pencil copying the same parts upon the paper in the lower half. It is evident that no more of this object can be copied at one time than can be seen by looking directly into the glass, of course the whole tree cannot be seen at once, and cannot be copied without shifting the instrument several times, so as to take it by separate pieces, which cannot be *seen at one time*, consequently there is great danger of losing the truth of the whole, while one is employed on each part.

REMARK by W. N.

IT is certainly the intention and instruction of the inventor of the camera lucida, that the tracing should be made upon that part of the paper where the picture and the point of the pencil can both be seen coincident, and not that a copy should be taken in the manner described by Mr. Sheldrake. This requires an attention to the small stop, which regulates the quantities of light which enter the pupil from the prism, and from the paper in the same direction; but I have not found it difficult to manage the position of the eye, which is the principal circumstance—and this will perhaps be as easily acquired by a few trials, as by any minute description of the process, which may be derived from Dr. Wollaston's paper in the 17th Vol. of our Journal, p. 1.

Method of drawing by the camera lucida.

VIII.

Remarks on some of the Definitions and Axioms in Barrow's Euclid. In a Letter from WILLIAM SAINT, *Esq.*

To Mr. NICHOLSON.

SIR,

Cromer in Norfolk,
August 4th, 1809.

IN reading over, a few days since, the 7th book of the English edition of Dr. Barrow's Euclid, several objections occurred to me against some of the definitions and axioms, which I noted down. On reviewing these objections, I must

Remarks on Barrow's Euclid.

must confess, that, *to me*, they appeared to have some weight; I resolved therefore to send them to you, accompanied with such *remarks* (for I dare not aspire to call them notes critical and geometrical) as appeared most applicable.

Submitted to the reader.

These objections and remarks, Sir, are submitted to you, and (should you deem them worthy of insertion in your widely circulated Journal) to your geometrical readers, with the greatest humility.

I am, Sir,

Your obliged and constant reader,

W. SAINT,

Of the Royal Military Academy, Woolwich.

Definition 6.

Definition 6.

" An even number is that which may be divided into two equal parts."

Definition 7.

Definition 7.

" But an odd number is that which cannot be divided into two equal parts; or that which differeth from an even number by unity."

Remarks.

Against these definitions it has been objected, that they are deficient in the word *integral;* which, it has been thought by some, should have been inserted between the words " equal parts" in each definition: for, it has been urged, any *odd* number is divisible into *two equal parts;* as 5, for instance may be divided into two equal parts $2\frac{1}{2}$ and $2\frac{1}{2}$. To this objection however these definitions are not liable, for number is defined to be " a multitude composed of units," and part to be " a number of a number:" therefore a part also must be " composed of units," and hence the objection is obviated.

Definition 8.

Definition 8.

" A number evenly even is that which an even number measureth by an even number."

Definition

Definition 9.

"But a number evenly odd is that which an even number measureth by an odd number." Definition 9.

There appears to be something erroneous in these definitions, since the same number may be found to apply to both of them; for instance, the number 40 is evenly even, because the even number 4 measures it by the even number 10; it is also evenly odd, because the even number 8 measures it by the odd number 5. These definitions would perhaps be less exceptionable, if expressed thus: *Definition* 8. A number evenly even is that which may be divided into two equal parts, having each part an even number. *Definition* 9. But a number evenly odd is that which may be divided into two equal parts, having each part an odd number. Remarks.

Definition 15.

"One number is said to multiply another, when the number multiplied is so often added to itself, as there are units in the number multiplying, and another number is produced. Definition 15.

This definition appears to be improperly expressed: for if, for instance, it were required to multiply the number 3 by the number 2, it is necessary, to obtain the product according to the definition, to add the number 3 to itself so often as there are units in the number 2, that is to say twice to itself; now the number 3 added *once* to itself gives 6, and added *twice* to itself gives 9; thus 9 would be obtained for the product of 3 multiplied by 2, which is evidently erroneous. Perhaps this definition would be better thus: one number is said to be multiplied by another, when it is taken or repeated as many times as there are units in that other. To those, however, who may be disposed to contend, that the words "taken or repeated" do not sufficiently define the operation intended; and who may farther insist, that multiplication is only *a continued addition*, Euclid's definition may perhaps be preferred, if the words *less one* be inserted after the word *multiplying*. Remarks.

Definition 23.

"One number is said to measure another by a third number, which, when it either multiplies, or is multiplied by the measuring number, produces the number measured." Definition 23.

This

Remarks. This definition seems to be objectionable on this ground, that it defines a number A, to measure another number B, by a third number C, when *either* C multiplied by A, or A multiplied by C, produces the number B. Now the possibility, that C × A can be equal to A × C forms the subject of the 16th proposition of the *very book* to which this definition is *prefixed*. To say the least, therefore, this definition is *out of order:* and as Euclid does not appear to have made any use of it, till after the 16th proposition, so certainly it ought not to have been given till the truth of the proposition virtually implied in it had been demonstrated; that is to say, till it had been proved, that C multiplied by A is equal to A multiplied by C, to which proposition it might have formed a corollary.

Axiom 7.

Axiom 7. "If one number, multiplying another, produce a third, the multiplier shall measure the product by the multiplied; and the multiplied shall measure the same by the multiplier."

Remarks. The first part of this axiom is admissible, since it only implies, that, if any number, A, be first multiplied by any other number, B, and then divided by the same number, B, the quotient will be A,—a truth which is evident from the opposite effects of multiplication and division. The latter part of this axiom appears to be objectionable, for it does not, like the former part, first suppose an operation to be performed upon a number A, and then the effect of that operation to be done away, or withdrawn by another operation of a *directly opposite nature;* for though by this latter part it is required to multiply A by B as before, yet it is not required afterward to divide by B, but by A: and though it may be an obvious truth, that A first multiplied by B, and then divided by B, will give A; yet it is by no means so obvious, that A multiplied by B, and then divided by A, will give B, for here the operations of multiplication and division are by *different numbers*. By the former part of this axiom, if B be first multiplied by A, and then divided by A, the result will be B; and if the latter part of it were *self evident*, namely, that A multiplied

plied by B, and then divided by A, would give B also, it would be $\frac{B\times A}{A}=\frac{A\times B}{A}$, or $B\times A=A\times B$; hence it appears, that the latter part of this axiom virtually implies the truth of the 16th proposition, and is therefore objectionable on the same grounds as the 23d definition.

Axiom 8.

"If one number measure another, that number by which it measureth shall measure the same by the units that are in the number measuring, that is, by the number itself that measures." Axiom 8.

This axiom implies, that if $\frac{A}{B}=C$, then $\frac{A}{C}=B$. Now this is really more of a proposition than an axiom. By the former part of the last axiom it may indeed be *inferred*, that, since $\frac{A}{B}=C$, A must be $=C\times B$; because $\frac{C\times B}{B}=C$; but, as it has before been shown, it by no means follows because $\frac{C\times B}{B}=C$, that therefore $\frac{C\times B}{C}=B$. This axiom therefore is objectionable upon the same grounds with the last. Remarks.

Axiom 9.

"If a number measuring another, multiply that by which it measureth, or be multiplied by it, it produceth the number which it measureth. Axiom 9.

This axiom implies, that, if a number A measures another number B by a third number C, then A multiplied by C, or C multiplied by A, gives the same product B; that is to say, this axiom implies the truth of the 16th proposition, and is therefore objectionable on the grounds before stated. Remarks.

Proposition 16.

As there has been frequent occasion to refer to this proposition in the preceding remarks, it may not be improper to observe here, that it is one of those which has engaged the attention of several eminent mathematicians of the present day, and among others the celebrated Legendre, who, in his "*Essai sur la Théorie des Nombres*," has given a demonstration

Proposition 16 has engaged the attention of many eminent mathematicians.

demonstration of the same; from which it may be concluded, that Mr. Legendre himself did not consider Euclid's demonstration of this proposition as *perfectly satisfactory.* Indeed it must be confessed, that in Euclid's demonstration, as given by Dr. Barrow at least, there is an air of *obscurity*, which renders it difficult to be understood. For the satisfaction of such of your readers as may not be in possession of an edition of Euclid containing the 7th book, it may be proper here to give both the enumeration and demonstration of this proposition, as they are found in Dr. Barrow.

Proposition.

Proposition 16.

"If two numbers, A, B, mutually multiplying themselves produce any numbers A B, B A; the numbers produced, A B and B A, shall be equal the one to the other."

B 4	A 3
A 3	B 4
AB 12	BA 12

Demonstration.

Euclid's demonstration.

For because $AB = A \times B$ *(a)* therefore shall 1 be as often in A, as B in A B, *(b)* and by consequence alternately 1 shall be as often in B as A in AB. But because $BA = B \times A$, *(a)* therefore shall 1 be as often in B, as A in B A; therefore as often as 1 is in A B, so often is 1 in B A; and *(c)* so $AB = BA$. W. W. D.

a 15 *def.* 7
b 15 7
c 4 *ax.* 7

Remarks.

With respect to this demonstration it must be observed, that the attentive student meets with a difficulty in the very beginning; for why does it follow, because $AB = A \times B$, that 1 shall be as often in A as B in A B? That $AB = A \times B$ is an identical proposition, and implies no more than that A multiplied by B is equal to A multiplied by B, from which no inference can be drawn. The next step of the demonstration, namely, "And by consequence alternately 1 shall be as often in B as A in AB, is deduced from the preceding by virtue of the 15th proposition, which proves, that, if 1 be contained in B as often as D is contained in E, then 1 is contained in D as often as B is contained in E. The demonstration proceeds with, "but because $BA = B \times A$, therefore shall 1 be as often in B, as A in B A." Now this

is

is objectionable upon the same principle as the first step of the demonstration. The next step is in these words: "Therefore as often as 1 is in AB, so often is 1 in BA." But this does not appear to be the most natural and obvious inference from what has been previously attempted to be proved; for, if it had been satisfactorily shown, that 1 is contained as often in B as A in AB, and that 1 is contained as often in B as A in BA, the natural inference it appears would be, that A is contained in AB as often as A is contained in BA, and so finally AB=BA.

From the objections here stated the following demonstration is easily derived, which is submitted to the consideration of the lovers of geometrical accuracy with the greatest humility, as seeming to afford a more satisfactory proof of the proposition than the one above given.

In this demonstration it may be proper to observe, that, to avoid any ambiguity, the sign of multiplication, or ×, should be read by the words *multiplied by*. It has been thought better also, instead of *referring* to the proposition, definition, or axiom, on which any of the steps in the process depend, to insert these at length.

Demonstration.

New demonstration.

Since by Axiom 5 "unity measures every number by the units that are in it, that is, by the same number," therefore 1 measures A, A times; and since by the first part of Axiom 7, "If one number multiplying another produces a third, the multiplier shall measure the product by the multiplied;" therefore B shall measure A×B, A times; hence 1 shall be as often in A, as B in A×B: but by Proposition 15, if 1 measures A as often as B measures A×B, then 1 shall measure B as often as A measures A×B, or 1 shall be as often in B, as A in A×B: Again by Axiom 5, as above quoted, 1 measures B, B times, and by Axiom 7, A measures B×A, B times, therefore 1 shall be as often in B as A in B×A; but it was shown above, that 1 shall be as often in B as A in A×B; therefore, as often as A is in B×A, so often is A in A×B: but by Axiom 4 "Those numbers, of which the same number, or equal numbers, are the same parts, are equal amongst themselves;" therefore B×A is equal to A×B. W.W.D.

IX. *Account*

IX.

Account of a New Acid, obtained from Ginger. In a Letter from a CORRESPONDENT.

To Mr. NICHOLSON.

SIR,

Acid from ginger.

BY the following process an acid (which I consider as new, and would propose calling the zingiberic) was obtained from ginger.

Process for obtaining it.

One ounce of the best white ginger was infused during two or three days, in six ounces of nitrous acid; after which rather more than an equal quantity of water was added, and the whole was kept at the heat of 212° adding water to supply the loss by evaporation, till the nitrous smell had disappeared. Carbonate of lead was then added to saturation, and the solution filtered. The lead was in the next place precipitated by sulphuric acid, and a second filtration was made.

Its properties.

By evaporating the filtered liquor, an acid, similar in appearance to short white pieces of raw silk, was obtained, which oxidates zinc and iron, and dissolves potash, soda, ammonia, barytes, strontian, lime, magnesia, and the oxides of zinc, iron, lead, and copper.

Its combination with magnesia.

The only farther account I can at present give of its salts is, that the (perhaps super-) zingiberate of magnesia has a taste intermediate between that of acetite of lead, and triple supersulphate of alumine.

Its difference from other acids.

The zingiberic acid differs from the sulphuric, sulphurous, carbonic, oxalic, tartarous, citric, mucous, succinic, and camphoric acids, in forming a soluble salt with barytes and lime;

From the nitric, nitrous, muriatic, acetic, acetous, sebacic, malic, and prussic, by remaining in the solid form at 212°;

From the benzoic and suberic, by its greater solubility;

And it does not, like gallic acid, precipitate copper of a brown colour.

A CORRESPONDENT.

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END OF THE TWENTY-THIRD VOLUME.

ERRATA.

Page	Line	
161	11	from bottom *for* A. Ibbetson, Esq. *read* Mrs. Agnes Ibbetson.
169	10	from bottom *for* A. Ibbetson, Esq. *read* Mrs. Agnes Ibbetson.
215	7	*for* preserved *read* pursued.
220	22	*for* as in seeds *read* as well as in seeds.
228	6	from bottom *for* determined *read* diminished.
350	5	from bottom *read* Fig. 13. Section just above the seed vessel. *a, a,* the calyx. *b, b,* the corolla. *c, c, c, c,* four stamens. *d,* the pistil. Fig. 14. Bottom of the seed vessel of the dianthus *a,* the calyx, &c.

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